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THE TREATMENT
OF
RHEUMATISM, EPILEPSY,
ASTHMA, AND FEVER.
BEING
CLINICAL LECTURES
DELIVERED AT
THE LONDON HOMŒOPATHIC

BY
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TO

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BELGIANS.

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CONSULTING PHYSICIAN TO THE LONDON HOMŒOPATHIC HOSPITAL,
ETC., ETC., ETC.

MY DEAR DR. QUIN,

Just one-and-twenty years ago the British Homœopathic Society was instituted. The Meeting was held at your house, and the attendance could not have been numerous. Two years afterwards, you delivered your first Annual Address to the small party of the disciples of Hahnemann, by whom you were

England, and to the promotion of which your time and talents have been devoted, will be the best evidence that your life of steadfast purpose has not been spent in vain ; and that the high social honours which crown its setting, while graced by wit, were won by merit, and are maintained by wisdom. That they may be long enjoyed, is the earnest desire of

Your sincere Friend,

J. RUTHERFURD RUSSELL.

33, CLARGES STREET.

April, 1865.

INTRODUCTION.

OUR RELATIONS WITH THE OLD SCHOOL.*

It is difficult even for the most charitable to vindicate the conduct of the great bulk of the medical profession in its bearing towards the minority which believes and practises in accordance with principles denounced, *ex cathedra*, not as false, but only as too exclusive. If the majority, thus intolerant of error, were firmly united by the bonds of a common creed and strong faith in the unity and infallibility of their own church, we could understand their dislike to all who, by denying what authority teaches, weakened their influence and power of usefulness. If the members of the College of Surgeons of Ireland had the same respect for the decisions of their College on all medical matters, that the adherents of the Roman Catholic Church have for the decrees of its Popes and Councils, then it would not be surprising to find that, having given up their right of private judgment on all points of practice, they should allow their College to overstep the rigid limits of its jurisdiction, and dictate to them on questions of professional morality. If this were the case,

* Slightly modified from an article which was published in the *British Journal of Homœopathy*, October, 1864.

we might lament, but could hardly censure, the wonderful occurrence which took place in Dublin a few months ago. An aged Archbishop was dying of *gangrena senilis*; and for the comfort of his family he wished to see a surgeon. His medical attendant was a member of the Dublin University and of the College of Surgeons of Ireland, and had done nothing to forfeit his professional status; yet because he prescribed medicines in different doses, and according to a principle only occasionally, not uniformly, adopted by the College of Surgeons, the surgeon, who was invited, declined to go and see the dying Archbishop, unless his Grace submitted to the ignominious condition of dismissing the physician who had been his comfort and support for years. The document deserves to be placed on record. It runs thus:—

“Mr. A. presents his compliments to Dr. Scriven, and in reply to his note just received, begs to say, that as his Grace the Archbishop of Dublin has decided that he will have no surgeon to visit him who will not meet Dr. Scriven in consultation, Mr. A. regrets that he cannot have the honour of prescribing for his Grace under circumstances which would be a direct violation of a recent ordinance of the College of Surgeons of Ireland, of which Dr. Scriven is aware.”

To this remarkable letter the Archbishop replied, that he was “so opposed to tyranny in any shape, that things must go on as they were.” That is, he de-

clined the attentions of the surgeon because he could not accept them without giving his countenance to tyranny; and the brave old man preferred to die true to himself, rather than give up the principle for which he had been contending during his whole life.

When Archbishop Whately stigmatised the conduct of the College of Surgeons as tyranny, he used an expression the propriety of which he had previously vindicated. In reference to the "ordinance" which Mr. A. treated with such respect, and which runs, that "no fellow or licentiate of the Royal College of Surgeons of Ireland shall consult with, meet, advise, direct, or assist any person who professes to cure diseases by Homœopathy," &c., &c., the Archbishop had written—"I was well aware of the detestable act of tyranny you refer to. I believe some persons were overawed into taking part in it against their own judgment. I have always protested against such conduct in all departments of life. * * * A man has a right to refuse to work except for such wages, or under such conditions, as he himself chooses to prescribe, but he has no right to *compel others to concur with him*. If there is any mode of medical treatment which he disapproves of, or any system of education which he thinks objectionable, he will be likely to keep clear of it of his own accord, without any need of compulsion or pledges. *Those, again, who may think differently ought not to be coerced or bullied.* * * * The truth is, the majority of mankind have no real love of liberty, except that they are glad to

have it themselves; but they have neither spirit enough to stand up firmly for their own rights, nor sufficient sense of justice to respect the rights of others. They will submit to the domineering of a majority of their own party, and will join with them in domineering over others. In the midst of the disgust and shame which one must feel at such proceedings as you have alluded to, it is some consolation to the advocates of the system denounced, to see that there is something of a testimony borne to them by their adversaries, who *dare* not trust the cause to the decision of reason and experience, but resort to such expedients as might as ably be employed for a bad cause as a good."

A college has no right, then, according to Archbishop Whately, to interdict its members from associating with those who think differently on matters of science from the majority of which the college is composed. But does the College of Surgeons or Physicians, or the Apothecaries' Company, think with any kind of approach to unanimity? When Homœopathy is contrasted with the old system—we avoid calling it allopathy, for many object to the designation—is it possible so to define the latter system except as that which is not *altogether* Homœopathy? There is much more divergence of principle and practice between different adherents of the old school, than between a large and important minority in that heterogeneous body and the School of Hahnemann. Let us illustrate our statement by an anecdote, and a quotation from the *Lancet*. The following incident

happened to one of our body; and we shall let him tell it in his own words:—

“We were talking of the change that is coming over the younger members of the profession, and of their tendency to approximate to us. I met with an interesting example of this the other day. Late one evening a lady called at my house, and begged of me to accompany her at once to see a child, which she represented as at the point of death. Accordingly I went with her, and was taken to the house where the little sufferer lay. I was shown into the parlour, and introduced to the medical attendant. He was a young physician, who received me with great politeness, and gave me the history of the case. The little girl was seven years of age; she had always been delicate. Some months ago she had been attacked with some intestinal derangement, accompanied with diarrhœa, and latterly, very alarming head-symptoms had appeared. The regular medical attendant of the family was not at home, and in his absence my informant had been called in. He treated the child with various mild medicines, but there was no decided improvement; rather the reverse. The old practitioner returned to town, came and saw the child, and ordered a large blister to be applied to the back of the head. To this my informant strongly objected; but the old gentleman was obstinate, would have, and had his way. Things went from bad to worse, and *la voilà!* I then went upstairs, and found the child insensible, with a very rapid, feeble pulse, enlarged pupils, and most decided squint. The mother, who

had long been a Homœopathist, but whose wishes in regard to the treatment had been overruled till too late, was in a state of utter distraction. She implored me to undertake the case, and do something for the child. On going down to the parlour, I told the young physician my opinion that the case was hopeless, but that I had no choice but to undertake it, and that if he liked I should be most happy that he should watch the treatment. To this he replied, that he should like to do so, but he was afraid it would give offence to the old practitioner; at the same time, he expressed his gratification at having met me. I said—‘From what you tell me, it seems to me, that in your practice you are much nearer me than the old gentleman whom you meet, and with whom you differ.’ My young friend smiled significantly, and, I believe, entirely agreed with my words. I then told him the medicine I gave, and I found him in the house next morning when I came. The case soon terminated as we anticipated; indeed there was no hope of the child from the first; but I confess I think there is hope of the young physician. At all events, recalling as I do the horror I have generally excited when brought into the presence of the *soi-disant* orthodox doctors, I could not but be gratified at so very different a reception in this instance.”

In the *Lancet* of last July there appeared a curious table. A medical practitioner, who had long suffered from hay fever, had from time to time consulted various medical men by letter; and he gives us, in a tabular survey, the opinions they gave him

of the causes of this disease, and the remedies, as follows:—

Consulted.	OPINION OF CAUSE.	RECOMMENDED.
Dr. A. ...	A predisposition to phthisis	Quinine and sea voyage.
Dr. B. ...	Disease of pneumogastric nerve	Arsen., Bell., and Cinchona.
Dr. C. ...	Disease of the caruncula	To apply Bell. and Zinc.
Dr. D. ...	{ Inflammation of Schneide- rian membrane	To paint with Nitrate of Silver.
Dr. E. ...	Strumous diathesis	Quinine, Cod-liver oil, and wine.
Dr. F. ...	Dyspepsia	Kreasote, Henbane, Quinine.
Dr. G. ...	Vapour of Chlorophyll	{ Remain in a room from 11 A.M. to 6 P.M.
Dr. H. ...	Light, debility, hay pollen	{ Ditto, Port wine, snuff Salt and Opium, and wear blue glasses.
Dr. L. ...	{ From large doses of Iodine } (never took any Iodine) }	Try Quinine and Opium.
Dr. M. ...	Disease of iris	{ Avoid the sun's rays from 11 A.M. to 6 P.M.
Dr. N. ...	Want of red corpuscles	Try Iron, Port wine, and soups.
Dr. O. ...	Disease of optic nerve.....	Phosph. Ac. and Quinine.
Dr. P. ...	Asthma from hay pollen	Chlorodyne and Quinine.
Dr. Q. ...	Phrenitis	Small doses of Opium.
Dr. R. ...	Nervous debility from heat	Turkish Baths.

“Herewith,” writes Mr. Jones, the correspondent of the *Lancet*, “I forward a synopsis of the opinions of a few of the most eminent men, in various counties, that I have consulted. I have substituted a letter for the name, as I do not think it prudent to place before the general reader the names of those who have so disagreed.”*

We admire the magnanimity of Mr. Jones; for after having been told that he had “a predisposition to phthisis;” that he was of a “strumous diathesis;” that he had “disease of the pneumogastric nerve;” “dys-

* It is commonly reported, in medical circles, that the reason why the treatment of the late lamented Prince Consort was never made public, was that it would have called forth so severe animadversion from the different members of the orthodox body of practitioners, as to have afforded a dangerous weapon in the hands of the Homœopathists.

pepsia;" "disease of the iris;" "disease of the optic nerve;" "disease of the olfactory nerve;" that he had had "phrenitis;" and was "poisoned by Iodine;" and that in order to better his condition he was to take "Quinine, Arsenic, Belladonna, Cod-liver oil, Kreasote, Henbane, Opium, Phosphoric Acid, Chlorodyne, soups and Port wine; to paint his nostrils with lunar caustic, and snuff Salt and Opium; to wear blue glasses; to remain at home all day and take Turkish baths;" we are surprised that he was restrained from taking all the revenge in his power upon his formidable friends.

Could any one seriously maintain that the addition of a few Homœopathic recipes would add to the incongruity of this list? Certainly not. Can one, then, for a moment pretend that the practitioners of the old system have arrived at such unanimity as to warrant their issuing a series of articles of faith, a disbelief in which shall disqualify the dissentient for holding professional intercourse or communion with the believers in the articles? The only commandments these colleges issue are like the latter portion of the Decalogue, and begin, "Thou shalt not"—What? Keep company with an unbeliever. An unbeliever in what?

There is a pleasant anecdote told of the first Earl of Shaftesbury. He was overheard by a lady, muttering to himself, "Well, well, all men of sense are of one way of thinking on matters of religion." The fair and innocent listener exclaimed, "Ah! my Lord, I have you now. Pray what may that one way be on which all men of sense agree." "That," said the

Earl, with a smile of inscrutable politeness, "no man of sense ever tells." We recommend the moral of this anecdote to our friends on the other side: they rival the clever Earl in the depth of their scepticism; let them equal him in the elegance of his tacit admission. But to unite total unbelief with fierce and rude denunciations of those who will not concur in the dismal cry of modern medicine, "There is no truth," is as unseemly as the rage of the atheist against those who, in spite of *his* entire satisfaction with the blank "eye-socket," the vacant orbit of the universe, persist in looking on the eye of the Almighty within the socket—the luminous orb within the orbit—and from their gaze derive comfort and guidance. Suppose *they* are wrong, and *he* is right, why yet should he be fretted because of their belief? So with our opponents.

"The only difference," said the late celebrated John Thompson, the biographer of Cullen, and predecessor of Professor Henderson—"the only difference between me and the Homœopaths is, that they give *very little* medicine, I give *none*." When such gentlemen are pressed, and free to answer, why they will not meet us, their reply is—"Because we look upon you as impostors. You pretend to do that which you know you don't do. You attribute to your doses effects which your common sense tells you they cannot produce. ~~This~~ This is the real ground of our avoiding you. You are tainted with a moral leprosy, and it is right you should be excluded from society, since we have not the power of shutting you up as you deserve." Such,

we believe, is the substance of what a vast majority of medical men in these islands think of us, and thus to themselves do they justify what the world calls gross professional illiberality.

We admit the sufficiency of the defence, if there were grounds to establish the justness of the accusation. If, trusting to nature and to diet, we amused our patients with globules, we should deserve the contempt and avoidance of all honourable men. But knowing as we do that the errors of Homœopathists are all the other way; that instead of attributing too little effect to mere medication, as a rule, they attribute too much, we feel confident that there will be a reaction in our favour, when the medical mind of this country is really satisfied of our honesty. Nay, we know that there is a reaction already. Hardly a week passes without our receiving a communication, in some form or other, from some one anxious to study Homœopathy. In fact, the receipt of so many inquiries has been the exciting cause of our taking up our pen at present, in the hope that we may be able to give some useful information to those who desire it; or at least acquaint them with the fact, that the reason we do not reply to their letters, is not that we are not deeply interested in their tentative efforts—their groping towards the light—but the great difficulty we feel in affording them any real assistance; and a deep conviction that the aphorism of Hippocrates, to the effect, “at all events do no harm,” is as applicable to counsel in the affairs of life, and in the region of speculation, as to the practice of medicine.

It is something for one who is now timidly feeling his way out of the old towards the new, to be made aware that he is not alone; that there are, at present in this country, many undeclared practitioners of Homœopathy. This may seem a contradiction of what we have just alleged as to the general state of the medical mind, and the intolerance of all medical organisations. But, as a rule, a minority has no public voice—especially what we may call a transition-minority—a minority thoroughly dissatisfied with its present position, but not so thoroughly satisfied of the one it thinks of occupying as to venture to leap the dividing chasm. Homœopathists, although a very small minority, have organs of opinion; and whenever any one fairly gets over to us, he is comforted by finding so many there before him. But before he comes out, while he still passes for an orthodox member of his body, his position is anomalous. In all statistical returns he would be classed among the allopaths; he does as they do; he is part and parcel of the bodies who act so rudely to us; he takes in the *Lancet*. But all this he does under protest, more or less loudly expressed, according to his courage and his temperament. The world at large, however, knows nothing of this protest; it has no time to analyse bodies and to appreciate fine differences. If he wishes to raise his voice, how can he do it? Write to the *Lancet*? It rejects his letter. And it acts wisely according to its generation. The organ of a party expresses its dogmas, not its doubts; it cannot afford to offend the bulk of its subscribers by even hinting to them, that the zeal they display against

Homœopathy, has the unfortunate *note* (to borrow the expression of Dr. Newman) of selfishness upon it. The description Elia gives of a true Caledonian, is more applicable to a model editor, whether of the *Lancet* or the *Times*. "His understanding is always at its meridian; you never see the first dawn—the early streaks; he has no flutterings of self-suspicion; surmises, guesses, misgivings, half-intuitions, semi-consciousnesses, partial illuminations, dim instincts, embryo conceptions, have no place in his mind or vocabulary. The twilight of dubiety never falls upon him. Is he orthodox?—he has no doubts. Is he an infidel?—he has none either. Between the affirmative and the negative there is no border-land with him; you cannot hover with him upon the confines of truth, or wander in the maze of a probable argument; he always keeps the path. You cannot make excursions with him, for he sets you right; his taste never fluctuates; his morality never abates; he cannot compromise or understand middle actions; there can be but a right and a wrong; his affirmations have the sanctity of an oath."

Between the Scotchman thus described and our model editor—say of the *Lancet*—there is this difference: in the case of Lamb's Scotchman, the self-sufficiency is real; but with the editor it is only assumed. Get your intolerant and dogmatic editor alone after dinner, over his wine and cigar, and ask him if he really means what he writes? The chances are he laughs in your face. "Mean it? of course not, personally. I know many Homœopathists who are as

good fellows as any going;* but suppose I were, on that account, to admit their letters, what would be the consequence? Why, one of two things. Either I should have to devote all my columns to the replies, or I should lose the bulk of my subscribers, and have the satisfaction of seeing the other Medical Journal, over the way, spring into sudden popularity by pretending to come to the vindication of the outraged consciences of the profession. No, no; we editors are like the later Roman emperors: when we are once raised on the shields of our comrades, we may brag and bully the outsiders *ad libitum*; but woe to us if we venture to check our faithful followers; we must let them loot as they like, so long as they sustain us up here, and help us to keep the barbarians at a safe distance."

So we say to our indignant neophyte, who, having been led to try Homœopathic remedies, has found them curing his old chronic cases which had baffled him for years—"Don't write to the *Lancet*. Your indignation at the abuse we encounter is natural; but it is in the nature of things that it should be so, and that the ferocity of our opponents should augment with the danger to their system. It is easy for us to indulge in expressions of reprobation of, and disgust at, the horrors of the war now raging in America. Should we be so mealy-mouthed if London were starving in consequence of one army of Irish at the mouth of the Thames, another at Reigate, and

* The position of a Homœopath with a practitioner of legitimate medicine, may be socially good.—*Lancet*, March 18th, 1865.

a third at Rugby? So long as the party exists at all as a party, it will use the same language, only its key will be sharper the nearer it approaches dissolution. The smaller the pipe the shriller the sound: when you hear it screaming like a railway whistle, prepare for sudden silence. But this will not be in our day."

Should our reader happen to be one of those for whom we specially write, a medical practitioner in the inceptive stage of belief, we may well imagine him exclaiming—"And is this all you have to say? Does all this introduction lead to nothing more than the advice *not to write to the Lancet*?" We pray your patience, gentle reader, and beg leave to assure you that there is more temptation to write to the *Lancet*—that is, to be angry and explosive towards your old friends—than you most likely are aware of. It is too much the fashion for every one, on adopting Homœopathy, to make a popular confession of the fact by the publication of a pamphlet. There was a time when our number was less, and when every addition was hailed with gratulation; as was, no doubt, each successive arrival in New England, after the tiny "Mayflower" had first deposited her crew on its shores. But as years rolled on, the number of emigrants increased, and their character changed; and, except under peculiar circumstances, few of them can now expect to be welcomed with jubilation. Besides, many of those popular confessions—by which term we do not mean that the pamphlets were popular in the sense of being eagerly bought by the public, but that they were (with few exceptions) addressed exclusively to the public—

had upon them the faults belonging to the first stage of love: the neophyte performed more wonderful cures in a month than the experienced practitioner in a year. In fact, his cures being to him all wonders, his exultation is natural; but it will save him much regret if he keeps his feelings on the subject to himself. If he publish in his callow stage, before he has had opportunity of verifying his belief by an extensive series of observations, or of ascertaining the limits of his powers, he will be somewhat dismayed, at a later period, to find he has committed himself to much he would fain unsay, but cannot. These observations do not, of course, apply to hospital physicians—men in the position held by the late Doctor Tessier, of Paris, or Professor Henderson, of Edinburgh; for men occupying public trusts have correlative public duties, and are bound to let the world know the reasons which induce them to abandon the usual, and adopt an unusual method of practice. And let me remark, that although these are, in some respects, most favourably situated, they are yet in very trying circumstances. Any hospital physician who dares to resist the suspicion and obloquy which await even the inquirer into Homœopathy, must have a certain heroic element of character—the basis, in fact, of all that is heroic in human nature—the devotion to an idea, to truth for its own sake, altogether irrespectively of the consequences to himself from the result of his inquiries. That there are such men among us now, we know and rejoice, not on account of the influence they are likely to exert in our favour, but because it is refreshing,

in an age of so much apparent baseness, to have the assurance, that below the dross there is true worth, true nobility—a deeper and higher life, which although pent up now, so as to be unknown and disbelieved in, will assuredly break forth and blossom, to the amazement of the generation of sceptics which denies the existence of any force but what can be gauged and manipulated.

But this is a digression; and we shall reserve any suggestions to this small but influential class till we have finished our homily to the pamphleteers. We should be very sorry that it were supposed we undervalued the good effects of popular tracts, like those of Dr. Sharp, Dr. Moore, and others. For their end, in their time, they were very useful. Written in a remarkably lucid style, they made Homœopathy intelligible to a great mass of persons who before were unacquainted with it. But even of so excellent a thing as tracts, it is possible to have enough and to spare; and we strongly advise converts neither to write to the *Lancet* nor to publish a pamphlet. But we shall tell them what to do if they are in the position of one of our correspondents, who writes to us in the following terms:—

“Allow me to ask your advice on the best mode of trying to possess myself of a knowledge of Homœopathy—practical and sound. I have obtained the diploma of —— and of the ——, and would now turn my whole mind to Homœopathy; and though I have had some experience, and possess, as it is, some knowledge of the subject, acquired during the last five or

six years, yet, encouraged by your former offer to aid me in any way in your power, I have thought of asking the benefit of your experience in this way. I propose staying in ——" (his native place), "and practising Homœopathy, although I have been invited, &c."

Although we have never seen the writer of this letter, and have only the slightest acquaintance with him by correspondence, it is impossible not to feel a keen interest in the modest and manly manner in which he sets about preparing himself for the great task before him. To such an one, if his health do not fail, success is certain; and he evidently will not be satisfied with a "settled low content," but will, for its own sake, aim at a high measure of excellence.

It is not in the nature of minds of the class to which Dr. C. belongs, and from which we welcome our best recruits, to be satisfied with a mere manual of practice. They desire a thorough knowledge of the system—how it arose, and how it has been modified, by the experience of its practitioners, during the last fifty years. To enable them to do so we should insist upon their acquiring the German language. For some time to come they will have leisure enough on their hands, and they will find, that the want of an acquaintance with the mother-tongue of Homœopathy, is a serious loss to them in many ways. The lack of German deprives them of access to a great many useful works, both practical and theoretical, which have not been translated; moreover, it prevents their reading in the original the provings of our best medicines, and thus getting nearer, as it were, to the spirit of the great

prover, the founder of our system. And the possession of this knowledge is a security against the tricks of those who quack it, of those who impose upon the credulous and weak by pretending to excessive zeal for the literal truth as taught by Hahnemann.

We shall suppose an acquaintance with German, sufficient to read, with the aid of a dictionary, the *Materia Medica* of Hahnemann, and other medical writings, which, on the whole, are not difficult.

Then we should advise the student, that, besides using Jahr's *Manual*, which is really an indispensable abridgment, he should, when a case is under consideration, refer to the original provings.

No doubt the abridgment will, on the whole, be found tolerably correct ; but by doing so he will fix the symptoms expressed in different languages more certainly in his memory, and will perceive between the medicines many shades of difference represented by Hahnemann, which no abridgment can convey. This study will give him an intimate practical acquaintance with the potential resources of his therapeutic treasury. He will then be curious to know the opinions of men of experience as to the best method of employing these means of cure ; and the questions of dose, of sequence, and of alternation of medicines will naturally press upon him.

In order to satisfy himself upon these important points, we should advise him to procure and study carefully a work by Dr. Hirschel, entitled, *Compendium der Homöopathie nach ihrem neusten Standpunct und Anleitung zum Studium und zur Praxis*

derselben. If our correspondent, or some one of equal ability and energy, would set about a careful abridgment of this work, we believe that the labour would be amply rewarded by the knowledge gained in its execution, and that its publication would be a positive boon to British Homœopathic practitioners. Some parts of the book would require to be translated literally; some we should be glad to see abridged; some perhaps omitted altogether, as being more suited to Germany than England; while, in the place of what we should leave out, we would put a summary of the opinions of the practical men of this country who have written on the questions under discussion. For, notwithstanding our great respect for the labours of the Germans, we are of opinion that, in the purely practical sphere, the British school of Homœopathy bears the palm; combining considerable acquaintance with the practice of our foreign colleagues, with a knowledge of what our countrymen do. We are free to confess, that if we had a dangerous illness, we should far rather be treated by certain of our own colleagues than by any others. We believe very few German, fewer French, practitioners are nearly so familiar with the experience of our school, as many in Britain are with the writings of the German and the French schools; so that, besides having more practical habits from its position among an intensely practical people, in an intensely practical age, the British school, in what relates to the development of the system of therapeutics bequeathed by Hahnemann, has enjoyed the advantage of a larger culture.

Another work, which would be worth a careful perusal, is one by Dr. Sorge, the author of a Prize Essay on Phosphorus, which we noticed on its publication. His last brochure, which we commend to the attention of the studious, is entitled, *Die Homöopathie befreit von Uebertreibungen und gestützt durch viele bewahrte Heilanzeigen*. A somewhat presumptuous title certainly—and quite in character! The author, in his preface, expresses a wish to have the opinion of his critics: evidently he has no doubt of his ability, in the event of their judgment being unfavourable, either to overthrow or condemn it. The great fault of this little book, is its outrageous violence of language in regard to persons whose offence we find it impossible to realise. For example, in the very full table of contents, we meet with this entry:—*A Winkler's neuester Unsinn*.—*A. Winkler's last nonsense*. The notice of Alfred Winkler's last nonsense runs thus:—“In the most recent time, a certain Dr. A. Winkler has arisen, for the purpose of serving up afresh the old Hahnemannian distinction between the primary and secondary medicinal action of drugs. He maintains, that, in the treatment of every internal disease, we are to give the dynamic remedy which corresponds in its primary effects with the symptoms of the case; so will the secondary action of the remedy, which is the opposite of the primary, and therefore also of the disease, be its cure.” This is *the nonsense*. To make its being nonsense self-evident, Dr. Sorge adds—“To tell them that W. (*sic*) is neither physician nor pharmacologist, but only a chemist, will doubtless suffice,

with many, to make them pitch this bit of a book into the fire (*das ganze Büchelchen kurz abzufertigen*). I must, however, in consequence of the attention it has excited, not from the Homœopathic side only, enter into a closer examination of its contents."

The longer we pause over this passage, the more difficult do we find it to frame a theory which shall account for the fact, that a clever man—and Dr. Sorge is a clever man—expects his readers to agree with him, that, because Dr. Winkler is not a physician, therefore his theory of the mode of action of Homœopathic remedies must be worthless—a proposition that sounds peculiarly strange to us, who remember how much was done to excogitate a satisfactory theory of Homœopathy by two men, neither of whom were either practical physicians or pharmacologists—John Fletcher and Samuel Brown; while the explanation itself, which Dr. Sorge treats with such high disdain, is one we should be very much inclined to support, were it not for the dread we have of seeing, in the next edition of his work, some such heading as this:—"The last arrival of nonsense from England;" followed by the quotation of a few sentences of this article, and a stinging word or two, to indicate the toss of our esteemed, but rash and intemperate, colleague's contemptuous head.

This extravagant style is not confined to Germany. In our English journals we have met with specimens of invective, piled one on the top of another, in such fashion as to produce a most ludicrous effect; and we know that, among men of letters, it is believed, that

the offensive scurrility which was the fashion in all controversial writing of a former generation, is now to be found only in the columns of medical periodicals.

We are taught by a high authority, that the use of extravagant language is a mark of provincialism, as urbanity is the sign of metropolitan culture. Style is now in the ascendant; and certainly, if any medical school were to distinguish itself by purity, precision, and moderation of language, it would thereby do much to advance its reputation. In our small body we have some excellent writers—men of classic culture. One we may mention without running any risk of appearing invidious; but we can hardly expect to see writing like that of Dr. Wilkinson, in his work entitled, *The Body in its Relation to Man*, except at rare intervals. A style so accurate, and yet so ornate, is, like music, a gift of nature, and cannot be acquired.

To return to Dr. Sorge. We the more regret that he should have been betrayed into the use of such improper language towards his colleagues, that his book, in a small compass, contains much deserving careful study. It also is well worthy of an abridged translation into English. Dr. Sorge is essentially critical; he is the opposite of a credulous observer, and, probably, carries his scepticism too far. He is no respecter of persons; even Hahnemann is not sacred in his eyes. Every proposition which will not stand his critical testing, is unceremoniously dismissed as untrue. In belief, he is a minimalist, accepting only a small portion of what is generally credited. His

medical creed is expressed in the following propositions:—

“1. The proving of medicines on the sound organism, *controlled by the result of their administration to the sick*, is the best way of establishing their curative powers. [For the italics we are answerable.]

“2. Those objective and subjective symptoms of a medicine are the most important and significant which are manifested by all or nearly all the provers, and which relatively have the longest duration. On the same level stand those symptoms which constantly recur to individual provers every time a dose of the medicine is repeated.

“3. The division of symptoms into primary and secondary, is artificial, and without practical utility.

“4. To ascertain the radical effects of a medicine, the simultaneous or successive manifestations of individual symptoms, and the aggravation or amelioration produced on them by cold, warmth, movement, rest, period of the day, &c., require to be known.

“5. The results of experiments on animals, as on dogs, often unavoidable in the determination of objective morbid groups, are of value as guides to the administration of the medicine in human diseases, only in so far as they do not contradict the results obtained in the provings on the human subject.

“6. The manifestation of the effects of each medicine must be carefully weighed, *to arrive at the particular disease* it represents in each tissue, organ, and part of organ it affects.

“7. Next in importance is the comparison of the

operation of peculiar medicines with that of other exciting causes of disease—for example, with the effects of exposure to cold, of being wet through, of anger, grief, terror, of a strain, &c., in order that a real similarity may be established.

“8. The application of the knowledge obtained by physiological provings of medicines, according to the principle *similia similibus curantur*, in the treatment of disease, has been proved to be blissful and healing by a thousand-fold clinical experience.

“9. In order to cure according to the principle *similia similibus curantur*, it is necessary :

“a. That the medicine administered should stand in a specific relation to the organ *primarily* diseased.

“b. That the medicine should specially affect the same part of the organ which was primarily affected in the case to be cured.

“c. That the kind of morbid action to be cured in an organ, or part of an organ, should not be essentially different from the disease excited by the medicine in a person in health.

“10. Within these bounds the choice of a medicine is so far free, that it is to be determined by its correspondence in subjective phenomena, by its relation to the disposition of the patient, by the exciting cause of the disease, or by the prevailing *genus epidemicus*.

“11. Paying attention solely to the bare collection of symptoms, as Hahnemann requires, without a definite diagnosis, is only justified by the impossibility of the other plan—[i.e., making a comparative diagnosis of disease and medicines].

“12. The medicine chosen according to the *similia similibus curantur* principle, must always be given by itself alone—never in combination with any other medicine. The practice, so common among Homœopaths, of the administration of two medicines in regular alternation, is nothing more than a gradual mixing of the effects of the one with those of the other, and will be had recourse to in direct proportion to the uncertainty in the choice of the medicine.

“13. The dose of a medicine chosen according to the *similia similibus curantur* principle, should be determined, as in all other methods of cure, by the specialities of the case under treatment; drops of the mother tincture being required when there is a torpid condition, and considerable dilution when there is much susceptibility.

“14. The diet, too, must be adapted to the peculiar conditions of the case, and of the medicines administered. It very often happens that very slight, or even no changes at all in the mode of living are necessary. The almost identical diet prescribed by Hahnemann for all cases, even the most dissimilar, is not only, for the most part, utterly impracticable, but entirely unnecessary, often hurtful, and altogether deficient in individualisation.”

We commend this “confession of faith” to the attention of Dr. Lippe, who, from the other side of the Atlantic, demands of Mr. Pope, “Who is a Homœopathist?” Mr. Pope, indeed, replies with so much spirit and propriety, that no other answer than his is required. Still it may be well for Dr. Lippe to be

made aware that there is a class in Germany as well as in England, whom not to designate Homœopathists would be to commit a solecism in language. Dr. Sorge avows his belief in the principle *similia similibus curantur*, as the guiding rule in the selection of medicines. He also believes—and shows his belief in a more satisfactory way than merely clamouring about it—in the necessity of proving medicines on persons in health; and he very strongly advocates the giving of but one medicine at a time, even to the condemnation of the alternation of medicines. Yet, believing all these points, which are the cardinal elements of Homœopathy, he does not even mention dynamisation or the Psora Theory. We are not now discussing the sufficiency of his dogmas; all we wish is to direct attention to them as representing a rising school of men distinguished by scientific culture and practical energy, who carry themselves somewhat disdainfully towards the older branch of the house of Hahnemann. Dr. Sorge is young and clever; he is, naturally, somewhat unpleasantly self-confident: when he has gone through the experience of life, and known disappointment, he will, we trust, have learned a little more reverence for the opinions of those who do not find it so easy to dismiss the perplexing questions suggested by the two words—Psora and Dynamisation.

Although we have quoted Dr. Sorge's articles of belief in full, we are very far from prepared to sign them. We entirely disagree with him in his repudiation of the doctrine of the primary and secondary effects of a medicine. In fact, we can hardly believe

that he dissents from this well-established physiological principle, although his words imply that he does. But it is for him either to defend so singular a position, or to decline maintaining it. For us, it is enough to point out that, if he really thinks thus, he differs from all the highest authorities on the subject.

Some of Dr. Sorge's theses, are highly suggestive, although, we think, rather ill-expressed. For example, the sixth seems to us to point to a method both of recording and reading a proving radically different from that adopted by many who have contributed largely to our *Materia Medica*. If we understand Dr. Sorge aright, we apprehend that he would always strive to ascertain the relation of the phenomena produced by a drug to those induced by the natural exciting causes of disease; and that he would refuse to recognise as symptoms those sensations which arose in the course of experiment, unless they could be traced to some morbid action in the system. He would deny a place in his *Materia Medica* to all symptoms which were not significant of known diseases. Now, if we had a perfect pathology, and were acquainted not only with all actual, but all possible forms of morbid action, nothing would tend to simplify and give exactitude to our *Materia Medica* more than the adoption of some such plan as this. But, in our present state of knowledge and ignorance, we fear that, with a great quantity of chaff, we should lose some precious grain. A proving is something like a prophecy, which remains a riddle till it is fulfilled. For instance, had Dr. Sorge proved Camphor at the time when Hahnemann

did, some of the peculiar phenomena which had no relation to those of any disease then known, might have been omitted, and their resemblance to those of the first stage of cholera might not have been so manifest.

But while we must have a very full record of all the well-authenticated, peculiar sensations produced with tolerable generality upon persons in health, who make experiments with drugs upon themselves, we heartily wish that none would do so but those who have the rare faculty of discerning between a *significant* and insignificant disorder of sensation—the former a symptom properly so called, a sign pointing to a morbid action—the other nothing, and yet looking like something, and the cause of endless blunders and difficulties. Between the lighthouses which should direct the mariner, play Will o' the Wisps, so like the true guides as to be almost indistinguishable. To rid our coasts of these false lights is the aim of the school to which Dr. Sorge, Dr. Roth, and others like them, belong. They will effect much good at the risk of some mischief.

The modification of the plan of merely proving medicines on persons in health, by the precautionary clause that the results obtained are to be qualified by the testing of their curative powers before giving them rank among our recognised therapeutic agents, is, in the present state of progress, probably a judicious restraint upon the admission of useless medicines. At all events, it is highly satisfactory to have the actual demonstration of the sufficiency both of the

à priori principle, *similia similibus curantur*, and of the accuracy of the observer.

The cases collected by Dr. Sorge to illustrate his conception of real cures, not mere recoveries, are, on the whole, well chosen for the purpose; and as many of them possess an independent interest for us, we shall let our readers judge of a few of them.

CASE XXI.

DR. SORGE'S *own observation.* *Painful Paralysis of the Arm cured by Phosphorus.*

"Meder, a master carpenter in Welton, 59 years of age, complained to me on the 6th of May, 1860, that for a year he had suffered from a most distressing pain in the right arm and shoulder. Early in the morning, on rising from bed, the arm felt paralysed and weary, sometimes from the shoulder to the elbow; it improved after work, and he felt nothing of it all day; but all the more did it distress him in the evening when in repose, although it did not disturb his night's rest. When he made unusual exertion, there was more paralysis, in consequence, for several days; and the upper arm was morbidly sensitive to pressure. The arm had not become thinner, and presented no outward manifestation. I gave nothing but five drops of the 4th dilution of Phosphorus, every evening, in water. After six days Meder returned. On the posterior surface of the lower part of both fore-arms and hands, a spotted, itching, erythematous eruption had appeared. I desired the Phosphorus to be discontinued, and in the course of three days, the erythema, and with it the

affection of the arm, had permanently left him. The patient had no recollection of having ever, before the pain in the arm, had any affection of the skin."

The form of rheumatic affection of which this carpenter was thus rapidly relieved, is usually very obstinate, and tends to become truly chronic; and we believe that the chance of the spontaneous recovery of such a case in the course of a week, is in reality infinitely small; so we have no hesitation in accepting this as a true example of cure by a well-proved and powerful drug. Whether there be any other explanation of the *modus operandi* of Phosphorus than the S. S. C. one, must be determined by those ingenious sceptics who, to avoid admitting the Homœopathic formula, propose explanations much more difficult to believe, and much less worth believing.

CASE XXIV. (page 80.)

Paralysis of the Bladder cured by Secale Cornutum.

By SOUCEROTTE.

"A forester, 50 years of age, of strong constitution, much exposed to the danger of catching cold, after having for some time suffered severe and enduring headache, fell suddenly to the ground, but did not entirely lose his consciousness. The treatment directed against this apoplectic seizure was without avail, and after three months he was taken to the hospital. On his admission he presented the following symptoms:—The patient was unable to walk; at each attempt his legs gave way under him, especially the right one;

just as little could he bend himself forwards or backwards; and when he sat upon a chair he fell over to the right side. The arms, especially the left one, were also weak, but not to the same degree; anæsthesia was at one time present; the intellectual faculties were in order, but there was general dulness, difficulty of speech, and inclination to weep. The patient complained of headache, and of pain in the loins, which, however, was not increased by pressure; there was want of sleep; the urine and fæces passed involuntarily; but, although the water of the enema was almost instantaneously expelled, there was frequent constipation for several days. The tongue, the organs of digestion and respiration were normal, except from occasional attacks of a sense of oppression of the breathing (owing probably to some paralysis of respiratory muscles). The pulse showed no feverish excitement. Arnica, Aloes, Ext. of Nux V., stimulating applications, &c., were employed with no result except a slight increase in the power of movement, and a slight decrease in the torpor of the bowels and the dulness of perception. On the other hand, the general condition remained sufficiently deplorable, and the state of the bladder was especially obstinate, undergoing no improvement from its original condition: so that the patient's bed was overflowed every night.

“On the 23rd of May, after the disease had lasted for six months, *Secale Cornutum*—at first in one grain, but afterwards in one-and-a-half grain doses, was administered three times a day; and, as early as the third day, involuntary passing of the urine ceased—

along with all the other symptoms of paralysis; so that the patient left the hospital on the 8th of June, perfectly cured, to go back to his place. For a year and a-half the patient has remained well."

This strictly Homœopathic cure was effected in an allopathic hospital, and published in the *Bull. gén de Thérapeutique*. This by no means detracts from its interest in our eyes, and makes it more likely to be accepted as a fact by the opponents of our system.

CASE XXVI.

Painful Serous Diarrhœa cured by Jalap.

By DR. KAFKA.

"An infant at the breast, three months old, had for three days bloody serous stools, mixed with much gelatinous mucus, and of a sour smell. The number of the evacuations was six during the day, and twelve during the night. In the day-time he cried only before and during the motion of the bowels; but during the night he cried almost without cessation, and drew the feet up to the body. The umbilical region was sensitive to pressure; there was clear percussion-sound over the whole abdomen; there was slight tenesmus; the anus was red and sore. Entire quiescence of all the symptoms within twenty-four hours followed the administration of 1-10th of a grain of the first trituration of Jalap every two hours."

The treatment in this case, too, although that of a Homœopathic physician, might have been equally well, so far as the medicine and dose are concerned, pre-

scribed by an allopathic practitioner from an ordinary druggist's shop.

CASE XXXI.

Intermittent Fever cured by Belladonna.

By DR. SORGE.

"August Rauchfuss, a boy 10 years old, in Cüllme, near Halle, was affected for five weeks, in the spring of the year 1854, with tertian ague. After a sense of drawing in the limbs, there followed shivering for an hour and a-half; the next three hours heat; and then, for some time, perspiration. China, in Homœopathic preparation, was of no more use than Bryonia. On my last visit I found that the boy complained, during the paroxysm, of headache as soon as he looked on water, and that, during the hot stage, there appeared before his eyes the same impression as if he looked at the sun. These indications induced me to give Belladonna in frequent doses. As early as the time of the second paroxysm there was entire remission, and I heard of no more attacks."

This is a singular and highly interesting case. It is to be regretted that Dr. Sorge does not mention what the dose of Belladonna was.

CASE XXXIII.

Chorioideitis (Acute) cured by Arsenic.

By DR. ALTSCHUL.

"Mr. H. G., of a tuberculous constitution, was attacked with severe inflammation of the eyes, in con-

sequence of continuously straining them by drawing at night.

“ Burning, stabbing, and boring pains spread from the eyeball over the supra-orbital region, to the head, and produced, in their almost periodic returns, nausea and retching. Great intolerance of light, and a contracted pupil, were combined with diminished powers of vision, and a sense as of looking through a veil. Professor Ryber, by the use of the ophthalmoscope, diagnosed not only distinct appearances of hyperæmia of the choroid, but likewise of congestion of the ciliary veins. Dr. Altschul so entirely cured this condition by the administration of Arsenic of the 6th dilution, that the sight was restored so as to suffice for the most minute drawing.”

We should have been better satisfied with this striking cure, if we had been informed of the length of time the affection had existed, and how long it was under treatment; also, whether any other means were employed than the internal administration of Arsenic. But even with these drawbacks the case is most interesting; for we know that such affections either do not pass away of themselves, or at least take a much longer time to do so than it is at all likely this one did.

CASE XXXVI.

A Cutaneous Affection cured by Conium.

By DR. SORGE.

“ H., a peasant woman, 64 years of age, frequently sickly, and affected with chronic bronchial catarrh,

sought my advice for the cure of an eruption, which appeared in autumn, and consisted of small ulcers and pustules, on the elbows, and upper and external side of the left fore-arm. They resembled in their form *impetigo figurata*. The pustules became transformed into ulcers, with purple, swollen, circumjacent parts, which, elevated in margins of irregular shape, and varying in size from a pea to a bean, easily bled when touched, discharged much matter, and, when dry, which especially happened when either kept warm or exposed to the air, became shrunk, and burned. The greater part of the external side of the fore-arm was cedematous, and felt cool.

“The only external application I employed was lappets of lard; and I gave five powders of Sulphur in trituration, one to be taken every two days. The only result manifest from this treatment was, that the pustules increased in size. Seeing this, I ordered Arsenic in very small doses; after which, instead of improvement, there appeared, on the external side of the wrist of the same arm, on the 11th of December, a new spot, congested with blood, and covered with pustules; the arm felt as if beaten, and was lame.

“I now ordered Conium Maculatum of the 3rd dilution, three drops a day. On the 19th of December all the ulcers were coated with thick brown scurf, with the exception of those on the olecranon. The Conium was continued at the rate of five drops a day.

“29th December.—The inflammation round the cicatrices is gone; some of the pustules are resolved,

and have passed into small, dry, painless nodules (Fleischknoten). Instead of the burning, the patient now feels only itching, and that but occasionally. There are scarcely any new pustules. *Pergatur.*

“30th January.—The cicatrices and nodules are gradually disappearing. The ulcers were covered three weeks later; and by the steady use of the Conium, nothing of the affection was left beyond a peculiar appearance of the skin.”

We believe that no class of cases is better fitted to afford positive evidence of the curative efficacy of Homœopathic treatment, than diseases of the skin. Easily diagnosed, frequently of long continuance, and not tending to spontaneous recovery, they sometimes disappear, as in the instance just quoted, with great rapidity under the use of the appropriate specific. We have almost always under our care cases which have been long under the most celebrated “skin-doctors” in Britain, and have derived no benefit, or, at best, a temporary one; and of these cases a considerable number get rapidly well from the commencement of the use of Homœopathic remedies.

CASE XL.

Angina Membranacea cured by Bromine.

By DR. SORGE.

“H. H., a girl of 11 years of age, after having suffered for four weeks with cough and hoarseness, which always increased by exercise, was at length seized with such dyspnœa, that the mother was obliged, on the 2nd of May, 1858, to administer an emetic, supplied

by a neighbour, in order to avert suffocation. The remedy gave only a few hours' relief.

"On the 3rd of May I found the little girl sitting up in bed, for she could only lie down for a short time; the difficulty of breathing was so great that the muscles of the throat and abdomen were in violent exercise, and *even the alæ nasi were visibly moved*. In the chest the respiratory sounds were only occasionally audible at particular spots; the voice was quite hoarse and metallic (*klanglos*); the cough was rough and croaking; the pulse was not quickened.

"While the mother went to fetch me, the child, to avoid being choked, had induced vomiting herself by putting her fingers down her throat. A surgeon (Böhme) who saw the case then, pronounced it fatal.

"I gave Bromine 2 (1·9 in distilled water), two drops from every half-hour to every hour, and nothing else.

"May 4th.—In the afternoon the breathing had become easier in the chest, and was more distinct; the cough, which, like the voice, had continued metallic, now rattled somewhat more loosely in the chest, and was now and then attended with expectoration. The child could now lie on her back for hours together, but was still obliged to compel vomiting with the finger. The medicine was continued at the rate of three drops in water every three hours.

• "May 5th.—On listening to the chest, the sounds are looser; there has been much expectoration; the patient, although she has not vomited, feels better. Continue three drops in water every two hours.

"May 7th.—The oppression of the breathing is now only slight; the voice becomes gradually louder; the patient expectorates much, and many lumps (*Stücken*), the expulsion of which induces retching; what was brought up looked sometimes red, and as if of blood. *Pergatur*, every three hours.

"May 10th.—The breathing is quiet and without effort; there is much expectoration, in which was a *membranous piece*, of a yellow colour, mixed with white mucus. The voice is still hoarse. *Pergatur*, four times a day.

"May 14th.—On this day I saw the little girl going about the house without any difficulty of breathing. Large mucous ronchi in the chest, and some hoarseness, were all that remained of the attack."

This case has some puzzling features. If it was really a case of membranous deposit in the larynx, how does it happen that the pulse was unaffected? If, on the other hand, it was a case of spasmodic croup, how are we to account for the expectoration of the shreds? We suspect some error in the observation in one or other of these points. Still we look upon it as a case of genuine cure by Bromine; all the more so, because we have met with very similar cases in some female patients, who were both hysterical and disposed to inflammatory affections. We had a case of what we should call *nervous* laryngitis under our care lately—by which term we mean real inflammation of the larynx, attended, in addition to the symptoms proper to this complaint, with exaggerated phenomena, such as choking, intense dyspnoea, &c.—which we

looked upon as sympathetic, the result of reflex action, not of direct suffocation. In this case we gave Bromine with excellent effect. In the narrative we have just quoted, Dr. Sorge notices the excessive play of the nostrils. This symptom has been the subject of much attention of late, in connection with a case of pneumonia, reported in the *Monthly Homœopathic Review*. The narrator of the case, who has the reputation of a considerable practice, lays great stress upon this symptom as an indication for Lycopodium. So far as our observations go, we are disposed to regard it simply as an indication of the excitement of the respiratory nerves of the face from any cause. It is best seen in a race-horse after the race; the wide, flapping nostrils indicating that all the apparatus which subserves respiration is brought into play. Next to that of the panting horse, perhaps the best example is in the hysterical girl when suffering from any impediment of breathing. In the case of pneumonia just referred to, the age of the patient, and many of her symptoms, suggest hysteria; and the recovery was probably, in great measure, due, first, to the lapse of time (for, as a rule, all simple cases of pneumonia in young persons get better of themselves); and, secondly, to the confidence the patient reposed in her medical attendant. We could not, we fear, get Dr. Sorge, and men of his school, to entertain the notion that a globule of the 200th of Lycopodium had anything to do with the result. Perhaps Dr. Sorge is too sceptical, or perhaps the admirers of Lycopodium are too credulous. Who shall say?

CASE XLIV.

Cheesy Pneumonia cured by Stannum.

By DR. SORGE.

"A teacher, 40 years of age, of middle height, addicted to the use of cigars, and, in moderation, of *lager* beer, and subject to abdominal and rheumatic affections, was taken ill in the beginning of May, 1856, with catarrh of the lungs, for which I prescribed various remedies in vain. By the beginning of June, I found that the catarrh had spread over the whole of the chest, but the finest râles were heard under both clavicles. For the breadth of a hand under the right clavicle, the tone was dull; while a consonating resonance was heard, both with the voice and breathing (*bronchophony and pectoriloquy*). The cough was distressing, attended with the sense of being beaten, and with smarting pain on the whole anterior surface of the chest; there was much yellow mucous expectoration, which left a persistent sweet taste in the mouth. The patient's tongue was coated with a yellow fur: he perspired much, and was very weak. By the 6th trituration of Sulphur, the only symptom which was improved was the sweet taste of the expectoration, all the other symptoms remaining the same.

"On the 16th of June I gave him the 6th dilution of Stannum, of which he was to take a dose three times a day. By the steady use of this one medicine, every trace of the morbid state of his lungs disappeared along with his cough."

We miss, in the record of this case, the state of the pulse—a singular omission. We may presume, however, from the perspiration and weakness, that there was fever present; and if so, we are disposed to look upon it as a case of arrested phthisis, and highly interesting. The locality of the dulness, the small mucous râles, the period of the year, the length of the duration of the attack, the perspiration and the languor, all look more like phthisis than pneumonia. Perhaps, however, by the term *cheesy* pneumonia, Dr. Sorge means soft tubercular deposit into the air-cells; in fact, the first stage of phthisis. The cure was very remarkable, and it was just the case for Stannum; only Stannum often fails, like everything else, even in just such cases, to give more than temporary relief.

CASE XLV.

Chronic Bronchial Catarrh cured by Carbo. Veg.

By DR. SORGE.

“A strong sailor, between twenty and thirty years of age, of a slightly cyanotic tinge of countenance, suffered for two years, in consequence of having taken severe cold, from asthma, combined with chronic bronchial catarrh. The cough was worse of a morning, and was attended with mucous expectoration only. It was increased by catching cold, and by all bodily exertion, as was also the dyspnœa. These exacerbations lasted, as a rule, for eight days, and were attended with fever in a moderate degree. Physical exploration revealed catarrhal râles over the whole

chest, most, however, on the left side. The liver was somewhat depressed. After having been long under various treatment, including Homœopathic, without having derived any benefit, he applied to me on the 29th of January, 1863; and between that date and the 13th of April, he took tincture of Sulphur, Rhus Tox., and Arsenicum, without any other effect than that the exacerbations were shorter, and unattended with fever.

“On the 30th of April I gave him *Carbo Veget.*, 6th dil., to be taken thrice, twice, and then once a day. With this medicine, which soon produced improvement, I persevered, with trifling interruptions, to the end of September, when the patient declared himself quite cured, since, for a long time past, he had had neither cough nor asthmatic attacks. Neither could I perceive any râles in his chest. I understand that, up to this time (February, 1864), he remains free from his former ailment. Although I consider a relapse as still not impossible, yet I consider that the present amendment is worth notice.”

To this we entirely agree. Such cases are very troublesome; and any well-marked effect of a medicine is interesting to the practitioner. But we miss the mention of what we should at the first have inquired into, had the patient presented himself to us—viz., the state of the heart. It is a singular omission, and one which vitiates, to a considerable extent, the value of the record.

CASE XLVI.

Cephalalgia Externa cured by Sulphur.

"A young married lady was cured of eczema impetiginodes by internal and external remedies. After the eruption had disappeared, drawing and stabbing pains were felt in the scalp. These pains were worse at night, and gave rise to the sensation of pain in the hair itself. The scalp was sensitive to pressure; and there was swelling of the lymphatic glands in the nape of the neck, and in the neighbourhood of the occiput. After this affection had lasted three months, I was consulted in the beginning of the year 1854. In the course of three weeks, by the administration of the 2nd centesimal trituration of Sulphur, the affection was permanently relieved, without the return of the eruption of the skin."

We have selected these cases, not because, to us, there is anything wonderful about them, but on account of their verisimilitude. They look true. We believe they will produce, on the minds of all practical men, the same impression which they produce upon ours. We are of opinion that they will be credible to practitioners of the old system, who will perceive that the cures were wrought for the most part by substances of acknowledged potency. Such medicines as Phosphorus, Secale Cornutum, Arsenic, Conium, Belladonna, Stannum, and Sulphur, are as familiar to them as to us. They are known to possess powerful properties. The only point in the case which may produce distrust of

the inference that the recovery is a consequence and not an accident, is the amount of the drug administered at each dose. This, however, is not so incredible as it used to be. And if our elder brethren once admit the reality of our positive cures to any degree whatever, the barrier of disbelief is broken down; and instead of blank denial and indifference, we shall meet with cautious inquirers—very cautious; too cautious as it may seem to our impatient, ready-made-up minds—still inquirers.

Any one who has watched the progress of Homœopathy since its rise in this country, must have been struck with the rapid outburst it made between 1840 and 1850. That was a decade of medical revival. By far the greatest number of men whose adhesion to the new school caused a sensation in the old, *came over* then. In the last fourteen years the system has spread widely; but few men of eminence have openly joined us. The first impression made by this fact is probably one of despondency. We believe the despondency is not warranted, and that a deeper knowledge of the latent power, now at work, would change our estimate of the real progress which our doctrines are making.

Let us observe, that this decade which brought over to us a distinguished Professor and clinical teacher, placed in the school which he adorned, a teacher of another type, under whom medical scepticism began its reign in the Edinburgh school. The writings of Professor Bennett are now telling powerfully. They teach the lesson he learned in France and Germany, that many, very many, of the most serious diseases

tend to spontaneous recovery under a judicious regimen—a belief which greatly enhances the difficulty of arriving at a conclusion in respect to the positive efficacy of any mode of medication. Some twenty years ago, when Professor Henderson began to treat cases of pneumonia, in the Royal Infirmary of Edinburgh, with Homœopathic remedies, the favourable result he obtained was accepted as a proof of the virtues of the medicines he employed. Now, however, it would not be so. The sceptics play the part of the magicians before Pharaoh; and for every wonder wrought in testimony of a Divine gift of healing, they produce a counterpart performed by their heathen goddess Nature, and the people's heart is hardened against the truth. The difficulty of testing the *positive* effects of our medicine, is enormously increased by the fact, that it is only by an accumulation of negatives, that we arrive at an affirmative in specific medication. We aim at the removal of morbid phenomena, so that nothing of them shall remain, by means which shall give rise to no indications. The theoretical perfection of our system is one of the hindrances to any efforts to test it.

Let us suppose, *what indeed we know to be a fact*, that a hospital physician desires honestly to put Homœopathy to an experimental test in the wards which he superintends: how is he to set about it? Two ways suggest themselves. First, as the Homœopathic method is one requiring minuteness of knowledge to a degree implying long study and experience, he may seek the assistance of some one skilled in the

system, and, with him, visit and prescribe for the patients. At the first blush this seems a good plan; but a little knowledge of the world will soon show why it will not work. Whatever the best heads and best hearts in the profession think (and we should not be afraid to be judged *by them*), we know that there is a *plebs medica*, who hold in their hands the rewards of the profession, and who rule, with plebeian violence, the higher natures whom they elevate, that they may use them. It is to the lowest class in the forum of medicine that our best surgeons and best physicians have to do ignoble homage. Some there are, we are proud to think, of too noble a character to submit to this degradation; but even they must, out of regard to their own usefulness, be very wary in their intercourse with any notorious heretic. Nor can we think that any Homœopathic physician, of much delicacy or modesty, would accept the post of *adlatus* to a hospital physician of the old school. That the Board of Management of any hospital could sanction the giving up of a ward to what they must regard in the light of an experiment, is simply preposterous.

A statement went the round of the papers about some enthusiast's having offered £1,000 to any hospital which would give a fair trial to our system. The generosity was ill-advised; for the offer could not with propriety be accepted; and the feeble clamour raised by a few disappointed persons against those who refused the offer was in bad taste, and found no response from the more respectable members of our body.

The post of physician to an hospital is an important trust. He who holds it is, in a sense, answerable to the public, to his colleagues, and to his pupils; and this threefold responsibility he cannot devolve upon another. If he introduce into his wards a Homœopathic physician, and get him to prescribe, in a case of pneumonia, we shall say, which proves fatal, who is to bear the blame? Not the Homœopathist, but his friend: and what an outcry there would be! The first blow following such a miscarriage would fall upon the hospital physician, and the second upon the disciple and school of Hahnemann. Indeed, we are sure that no good, and much mischief, would come of introducing an avowed Homœopathic practitioner into an hospital, before the majority of the supporters of that hospital place him there by open suffrage.

Abandoning, as impracticable, the direct personal assistance of a skilled Homœopathic practitioner, our inquirer will bethink himself of the next best plan of testing the system; and although he cannot introduce into his wards one whose presence would be obnoxious, there can be no reason against his consulting with him in private, as to whether the Homœopathic treatment of some diseases might not be so simple as not to be beyond the attempt of his own experience. But the Homœopathist would reply thus: "You seek an impossibility. The very essence of Homœopathy is a rigidly exact correspondence between the fundamental features of every given case and its appropriate remedy. We ignore *nominal* diseases; we do not admit the existence of such abstractions; we only recognise sick

to you must not be taken as indicating our treatment of these cases. Your position is peculiar, and you must accommodate yourself to its peculiarity. You have not our preparations in your hospital; you must do your best with your own. We believe you will do some good, but not so much as might be done."

Having thus guarded ourselves against misconstruction, we should make the following suggestions for the treatment of the four diseases we have named:—

1st. Acute rheumatism. *Till the pulse falls*, give the following prescription:—

R. Tinct. Aconit., gtt. x.
Aquæ distill., ℥ viii.

Signa. Sumat unciam unam tertiâ quâque horâ.

After the pulse has fallen decidedly—

R. Tinct. Bryon., gtt. x.
Aquæ distill., ℥ viii.
Sumat unciam unam ter die.

2nd. Pneumonia. In all cases of uncomplicated pneumonia, give the following prescription:—

R. Phosphori, gr. i.
Spirit æther sulph.
Spirit vini rect., a a 3 ii.
Aquæ distill., ℥ xii.

Sumat drachmam unam quartâ quâque horâ.

3rd. Pleurisy. If there is much fever—

R. Tinct. Aconit., gtt. x.
 Aquæ distill., $\frac{3}{4}$ viii.
 Tinct. Bryonia, gtt. x.
 Aquæ distill., $\frac{3}{4}$ viii.

Signa. Sumat unciam unam singulæ misturæ 4tâ
 quâque horâ vicissim.

If there is little or no fever, omit the Aconite.

4th. Bronchitis. If there is much fever—

R. Tinct. Aconit., gtt. x.
 Aquæ distill., $\frac{3}{4}$ viii.
 Solutionis Arsen. Fowl., gtt. v.
 Aquæ distill., $\frac{3}{4}$ x.

Sumat semiunciam 4tâ q. q. h. vicissim.

If there is little or no fever, omit the Aconite.

We are well aware that, in suggesting this treatment, we are proposing a compromise, or perhaps we might say, accepting the proposal from the other side. Whether it is allowable to do so, may be a question with many. And even if such a compromise be not condemned on abstract grounds, exception may be taken to the particular treatment we propose. In this case, we should be only too glad to receive suggestions, especially from those who are familiar with the working of hospitals.

Many of our most esteemed colleagues will object to our proposal of giving two medicines in alternation. We concede that it would be much more satisfactory if we could avoid doing so; but, in this rough world,

we must constantly content ourselves with adopting the most practicable and expeditious method, even at some expense of scientific principles. It is better that a man should live on bad food which he will eat, than die because he won't eat good food; and it is better that Homœopathy, even in its roughest and coarsest embodiment, should gain any admission into the magnificent hospitals of this country, than that it should be confined within its present narrow territory.

But it may be asked, what we expect to be gained by the experiments we suggest.—We believe that if one hundred cases of each of these diseases are carefully watched, and the results tabulated, they will be found to be the following:—

1st. Rheumatism. The pulse will fall, and the pains abate, under the use of Aconite, in decidedly shorter time than in cases left to nature. The average duration of the cases will be materially shortened. There will be fewer heart-complications; and if these occur, they will pass off without leaving permanent mischief behind.

2nd. Pneumonia. The dyspnoea will sensibly lessen almost from the first. The pneumonic infiltration will be at once restrained, and there will be no increase of dulness; but the powers of resolution will set in, and the cure be completed at an earlier day than in cases left to themselves. The mortality of the hundred cases will be less, by a considerable number, than that of a hundred cases of *equal severity* left without treatment.

3rd. Pleurisy. The pain will rapidly yield after

the administration of Bryonia, and there will be little effusion if it has not already begun; if it has, there will be little increase. At the same time it is right to observe, that Bryonia is chiefly suitable for active inflammation of the pleuræ, and not for passive effusion into their cavity.

4th. Bronchitis. There will be relief to the dyspnœa; the character of the expectoration will improve, and it will be more easily brought up.

There are many other acute diseases which we should like to add to this list—peritonitis, dysentery, hepatitis, &c.—but it would require a long time to collect a sufficiently large number of cases; and there are, in this country, very few physicians who would venture to leave a corresponding number of these cases to the unaided efforts of Nature, however much it has become the fashion of late to deify this abstraction, and frame, out of a mass of independent laws, an idol such as rude pagans worshipped, before science revealed what natural laws really are, and before man—in right of his “being a-kin to God in his spirit,” as Bacon puts it—recognised his true place, as not of the earth, but only on the earth; supernatural as well as natural, and, in his supernatural capacity, above, and not under, the so-called laws of nature.

Suppose, then, one hundred cases of each of these four diseases treated according to the formula we have suggested, and the result what we anticipate, and so sufficient evidence procured of the superiority of this treatment over that of doing nothing at all—what

have we gained? The experiment will occupy, probably, some years: at the end of that time, taking the most hopeful view of the trial, the following conclusions are all we shall have arrived at:—In acute rheumatism, Aconite, followed by Bryonia, is of positive service; so, in pneumonia, is Phosphorus; in pleurisy, Aconite and Bryonia; and in bronchitis, Aconite and Arsenic. What the better should we be of such a verdict? Is it proved that these medicines act in virtue of any Homœopathic relation to the disease for which they are administered? May not the Aconite act in rheumatism as a sedative, calming the action of the heart? the Phosphorus as a restorative, giving vital energy to the lungs? and the Arsenic as a tonic? If this were admitted, it would only leave the Bryonia to be accounted for, and this might act analeptically—who knows? To this we should reply—"If you have got nothing else, you have got some good empirical remedies for very dangerous and painful diseases: so the time occupied in the experiment is well spent."

We should go farther, however, and direct attention to two facts: we would say—"1st. Observe the dose, and you will see that it is so small as not to be capable of producing any general physiological action, such as is understood by tonic, &c.; and the good it has done must have been effected by specific action on the seat of the disease: there, then, are four specifics for four morbid states."

"2nd. Recollect how these remedies have been obtained; neither by conjecture nor by accident, but in

virtue of an hypothesis, that because they excited certain effects on persons in health, therefore they would cure similar phenomena in the sick. These particular medicines were selected in preference to all others, in obedience to a formula; in the four instances in which you have tried them, the *à priori* anticipations have been verified by the occurrence of the predicted results. It may have been accident—a series of remarkable coincidences, such as often perplex us in our attempts to solve so complicated a problem as that of determining the efficient cause of any effect, towards the production of which many actions might be suggested as possible causes. But, admitting this, would it not be the part of a wise man to give the formula a further trial? Is there not presumptive evidence in its favour? Medicines so chosen, and given in such quantities, are innocuous; and have you not, in your private and public practice, plenty of cases which are making no progress, and in which you are fully justified in trying a perfectly harmless experiment? If you have—and who has not?—then take a good manual of our provings. Suppose, to begin with, you get the first part of the Hahnemann *Materia Medica*, published by Bailliere, containing the proving of the Bichromate of Potash, of Arsenic, and of Aconite, and test these three medicines. If you do so, I will tell you what will happen: you will meet with a succession of most agreeable surprises; you will find, as it were, a sudden sunshine spread over your practice. Many cases of lingering illness will rapidly show symptoms of improvement; still more acute and pain-

ful disorders, not necessarily dangerous, such as sciatica and neuralgic affections, will be almost instantaneously cured. You will feel your powers of healing enlarged beyond your wildest hopes; and, as one instance follows another of the successful application of this divining rod left us by Hahnemann, you will gradually experience the growth of that strongest of all convictions, which, resting on no communicable facts, results from the perpetual accumulation of atoms of evidence—atoms that, eventually combining in obedience to certain undiscovered laws of our mind, build up an adamantine foundation of certain faith, far more unquestionable to us than the belief which we arrive at by even mathematical demonstration.”

It is to the occurrence of such agreeable surprises in their practice, when, induced either by the pertinacious advice of some unprofessional Homœopathist, or by the reading of some vigorous appeal, or, in blank despair at having exhausted all the known methods in vain, the ordinary practitioner puts some of his least promising cases under our treatment, that we owe a large number of unavowed believers in Homœopathy. And what are they to do? In the meridian of life, for the most part, with well-established practices, not much more than just sufficient to meet the great expenses that are involved in maintaining their own and their families' position in London or some other large city, are they to run the risk of losing their patients by an open profession of the faith of the heretic? However honest their convictions, it must yet require the greatest judgment to determine the

proper time for moving to our side. Long before the time arrives—how long will depend on an infinite number of circumstances and considerations—they will be with us in conviction, and anxious, as we can testify from experience, for all the practical assistance we can give them. To this class, and it is a considerable class—much larger than is generally supposed—we would recommend that the transition from the old school to the new should be very gradual. Let them, at first, be content with making themselves well acquainted with the virtues of a few of our best-proved medicines, and with applying their knowledge in well-selected cases. Let them get the large edition of Jahr's *Materia Medica*, with the clinical notes of Noack and Trinks, and this

Nocturnâ versate manu, versate diurnâ.

The errors of Jahr, especially of his Repertory, arising chiefly from the original imperfection of his medical education, are more likely to offend than to mislead an experienced practitioner.

From this important class we do not expect much assistance in the further development of our system. They will find their time amply occupied by the greater amount of study required, and by the almost certain increase in the extent as well as success of their practice. It is enough if they introduce the system to the notice of new circles; and if, at the proper time, they openly announce that, after a sufficient experimental investigation, they are satisfied that the Homœopathic method of practice is superior

to all other methods. Testimony so disinterested, coming from such a quarter, is unimpeachable, and will make an impression on all persons of candour and intelligence, and of courage enough to think and act for themselves.

Lastly, we would say a few words to students. First, let us assure the students who are convinced of the truth of our system, and are anxious to practise it, that they have nothing to fear from the examining boards on account of their faith. We are assured by those who have recently gone through the ordeal, that even when the examiners know the predilection of the examined for Homœopathy, they do nothing to entrap him, but merely ask questions which test his knowledge, not his faith. To one practitioner who asked the president of the College of Surgeons, whether his being a Homœopathist would be a bar to his receiving a diploma, the answer given was, in substance—"We don't care a straw what you believe—you may believe in magic if you like—only know your anatomy and surgery, and you will pass." So, to our student friends we say—know your anatomy and surgery, say nothing about Homœopathy—don't parade it—be conspicuous only for diligence; and there is no medical board on this side St. George's Channel which will reject you. And after you have obtained your diploma, do not be in too great haste to settle in practice. You will be enticed by invitations from places where you would be sure to earn a comfortable competence; but if you get into practice too soon, you will be dwarfed for life; you will find that you have little leisure, little in-

clination, and no necessity for study; and that in the exercise of the merest routine of Homœopathic practice, you will easily distance your allopathic competitors. Hence you will be very apt to take credit to yourself for your success, instead of giving it where it is due—to those who, by intense labour, forged for you the key which you find so powerful in opening the locks of Nature's treasury.

But if, instead of being satisfied with obscure ease, students set before them the duty of developing the system as well as of supporting themselves—or, to use a quaint German phrase, of foddering as well as milking the cow—then, indeed, we should entertain the highest hopes of medicine. We would have them attend the London Homœopathic Hospital, or some well-managed dispensary, to furnish themselves from the existing armoury with ready-made weapons: but besides doing this, we would have them institute critical provings, either of medicines already proved or of new ones, and devote themselves to the reconciliation of the problems suggested by discoveries in pathology, with the results obtained by experiments with morbid agents of all kinds. Here is a field of infinite magnitude and of inexhaustible fertility: all it wants is cultivators. We have among our body more than one medalist: what are they doing? Are they satisfied with the lower prizes obtained at the University? They must know well that there are within their reach prizes of infinitely higher value—the prize, indeed, of the development into the perfect medical art of that system of

which, were Hahnemann to reappear among us, he might say:—

“Once in a golden hour
 I cast to earth a seed,
 Up there came a flower
 The people called a weed.
 To-and-fro they went
 Thro’ my garden bower,
 And muttering discontent,
 Cursed me and my flower.
 * * * * * *
 Most can raise the flowers now,
 For all have got the seed,
 And some are pretty enough,
 And some are poor indeed.
 And now again the people
 Call it but a weed.”

It is for us—for our students especially—to determine whether our system shall deserve to be called the great weed or the great flower of medicine. All that we require of them is zeal—the vitalising element which keeps alive that ideal, the persistent pursuit of which constitutes the fundamental contrast between the noble and the ignoble character.

LECTURE I.

ACUTE RHEUMATISM.

I SHALL begin this lecture by reading two well-marked cases of this most formidable disease, which were treated here last summer. The first case is that of

Mary B——, aged 19, a servant girl, unmarried, admitted into the hospital on the 3rd of June, 1861. She states that, after having for some days suffered from pain in the head and face, she was suddenly seized, three days before her admission, with severe pain in the ankle-joint and calf of the right leg, which soon extended downwards to the foot, and upwards to the knee of the same side. The pain had steadily increased in severity. On her admission she complained of intense pain in the right ankle and knee-joints; the pain extended to the hip. Both joints were red and swollen, and the slightest movement greatly increased her sufferings. The pain was constant, although more or less severe. The pulse was 140 in the minute; full and sharp. The skin was hot and moist. There was great thirst; the tongue was covered with white fur the urine was scanty, and high-coloured.

* R. Acon., 3rd dil., gtt. j
Bryon., 3rd dil., gtt. j.

S. Alternately every three hours, and to have the affected joints wrapped in cotton wool.

* Prescribed by the house-surgeon before my visit.

June 4th.—Complains of pain in the left ankle and knee, which are also swollen. Skin hot; great thirst; pulse 140 in the minute.

R. Acon., 3rd dil., gtt. j.

S. A dose every three hours. This was continued till

June 7th, when the pain in all the joints was much less severe; the pulse was 104; a slight systolic murmur audible; there was gentle perspiration; the tongue was moist and coated.

R. Bryon., 2nd dil., gtt. j, a dose every four hours.

9th.—Pain very much better in ankles and knees, but complains of painful swelling of left wrist and shoulder.

Continue medicine.

12th.—Pain almost gone. Pulse 100; bruit no longer audible.

Continue medicine.

14th.—Free of all pain except in the right knee, and there very slight. Pulse 92.

17th.—No change.

Sulph. O., gtt. j, a dose three times a day.

19th.—Slight return of rheumatism in right hand and shoulder, with some swelling.

R. Bryon., 2nd gtt. j, a dose three times a day.

21st.—Quite free of all pain. Pulse 68 ; sounds of heart perfectly natural.

The next case is that of a man, Daniel R——, aged 43, admitted on the 11th of June.

He states, that fourteen years ago he had an attack of acute rheumatism, but has not suffered from it since till seven days ago, when he was seized with severe cramp-like pain in his right foot. On the following morning both ankles were painful and red, and swollen ; thence it extended to both knees, both shoulders, and both wrists.

On admission he was perfectly helpless, suffering intense pain in the wrist and finger-joints of both hands ; both knees and both ankles were red, swollen, very tender to the touch, and in constant and severe pain, much increased by the slightest attempt at motion. He complains of slight pain over the cardiac region. On applying the stethoscope, distinct rubbing sound is perceived at the apex of the heart. The valvular sounds are muffled and inaccurate. The pulse is 100 in the minute. There is great thirst. The tongue is white and coated ; the urine is high-coloured and scanty ; the skin hot, and covered with copious sour perspiration.

R. Bryonia, 3rd dil., gtt. j, every three hours.

13th.—Pain still very severe ; some oppression of breathing ; slight but distinct systolic murmur. Enlarged area of dulness over the heart. No rubbing sound audible.

Continue Bryonia.

14th.—Pains much better to-day. Moves all his limbs freely, except left leg.

Continue Bryonia.

19th.—Much better. No pain at all in lower limbs, and but slight pain in shoulder and wrists. Pulse 72, steady and regular. Still slight systolic murmur; no rubbing sound; lessened dulness over heart.

Continue Bryonia.

27th.—Slight stiffness and pain of right thigh and left shoulder; otherwise well. Still some systolic murmur.

Arsenicum, 3rd dil., gtt. j, three times a day.

July 3rd.—Convalescent. Heart's action normal.

These two cases may be taken as fair specimens of the general course of acute rheumatism, treated according to the Homœopathic method. When we consider the severity of the symptoms, and that improvement commenced in both cases within five days; that both may be looked upon as convalescent within eighteen days of their admission; that both were complicated with affections of the heart, more or less severe, we are entitled to assume that the recovery was due to the remedial appliances employed, and that, in short, the cases *were cured*. To suppose that, in the same month, in the same place, two cases of acute rheumatism recovered spontaneously in such a period of time, is doing violence to all laws of probability. The general duration of such cases is thus described by Dr. Fuller,

in the third edition of his able treatise on *Rheumatism and Gout*, at page 70:—"The duration of an ordinary uncomplicated attack of acute rheumatism has been variously stated by different writers. Dr. Warren reported six weeks as its ordinary duration; Sir Charles Scudamore stated, that, 'in a case of which the issue is favourable, the fever and pains are brought to a stand *at the end of the third week*, and in slighter attacks at an earlier period; but when the course of the disease is untoward, a period of two months scarcely serves to exhaust its power of producing even acute symptoms.'" M. Chomel's experience led him to believe that four weeks is about the average period for arriving at convalescence, and *that recovery never takes place before the twentieth day*;† and Dr. McLeod imagined, that with the common methods of treatment, probably five or six weeks may be about its duration.‡ My own observation," continues Dr. Fuller, "has led me to believe that, even when unattended by any internal affection, the disease, under ordinary methods of treatment [surely we may add, and under no treatment at all], endures very generally from four to five weeks; and, under unfavourable circumstances, may be indefinitely protracted. Of the 246 cases admitted into St. George's Hospital, under the care of the physicians during the time I held the office of medical registrar, the great majority were decidedly convalescent about the end of the fourth or the beginning of the fifth

* *On Rheumatism*, p. 25.

† *La Lancette Française*; Août, 1834.

‡ *On Rheumatism*, p. 25.

week from the commencement of the attack, and were ready to leave the hospital about the end of the sixth week." Assuming, then, that the two cases were cured by the remedies (and, I may observe, that it would be easy, if further proof were required on this head, to narrate many similar cases; indeed, we shall have to do so in the sequel), let us now consider *what* it was that was thus cured, and *how* the cure was brought about.

What is acute rheumatism? This is a question to which the answer is very different since the modern revival of humoral pathology. We are taught by the researches of the chemist, and the experiments of the physiologist, that the disease known as rheumatic fever, or acute rheumatism, depends upon certain altered conditions of the blood. Upon this new pathology, a new therapeutic method is founded; and both are well worthy of our attentive study. The pathological doctrine advanced by Dr. Prout, and advocated by Dr. Todd, Dr. Fuller, and others, that the cause of acute rheumatism is an excess of lactic acid in the blood, has been subjected to experimental examination by Dr. Richardson, whose treatise on the coagulation of the blood is a model of inductive reasoning as well as clear exposition. Dr. Richardson made a series of experiments, to ascertain the effect of introducing into the circulation, by absorption as well as by direct injection, a quantity of lactic acid. The conclusions he arrived at he thus expresses:—"The first inference deducible from the experiments, according to my reading of them, is, that lactic acid has the

power, when existing in an animal body in excess, of producing a class of symptoms attaching themselves mainly to the fibro-serous textures, and which, regarded in all points of view, are essentially the symptoms of acute rheumatic inflammation. The symptoms are obviously dependent on the presence of the poison. Experiment 304, which is, in fact, a series of experiments, tells this fact definitely. By giving time for the elimination of the poison, symptoms could be made to disappear; on the re-supplying of the poison, symptoms returned; the alternation was as marked as night and day, and was as clearly in the order of natural sequence. There was also what the old-school men would call metastasis of symptoms: now one joint suffered, then another; again the heart. Thus, it is clear that lactic acid has the properties of a rheumatism-producing poison; but that, in the human subject, it is the veritable cause in every or any case of rheumatism, is only an inference. As yet, there are wanting many experiments to make the matter a demonstration. But this seems to me clearly proved—that if not lactic acid, yet some acid of analogous character, is always the cause” (p. 300). The experiment referred to affords so important a contribution to the proving of lactic acid, as to deserve our attention, independently of its forming a main link in the evidence upon which the verdict against lactic acid is formed. Although a somewhat long narrative, I think it right to extract it without any attempt at condensation, beyond the omission of an irrelevant sentence:—

“Into the peritoneum of a healthy dog (the respiration and heart-sounds being perfectly natural) I injected, through a lancet opening, one ounce of a solution of lactic acid, with two ounces of water.

* * * On the following day the dog was dull, heavy, and cold, suffering from repeated shiverings. There was no pain on pressure over the abdomen.

“*Third day.*—The temperature of the body had very much risen. He was thirsty, and suffered from great pain in all the limbs, shrinking whenever he was touched. There was also well-marked injection of the vessels of the sclerotic. The pressure of the stethoscope over the region of the heart seemed to produce great pain and anxiety. The first sound was obscure, the second sound accented. The action of the heart was irregular and quick. On this day another ounce of the acid was thrown into the rectum, but was returned.

“*Fourth day.*—The injection of the sclerotic was intense, and the pain in the limbs seemed to have disappeared, except about the shoulders. The heart-sounds were as on the previous day. One ounce of the acid was administered by the mouth, and was retained. The animal was much purged this day; the fæcal matter had a strong acid reaction.

“*Fifth day.*—The purging continued, and the fæces had still an acid reaction. The animal seemed greatly relieved; the heart-sounds being clear, and the beats regular. An ounce of the solution of the acid, diluted as before, was injected into the peritoneum.

“*Sixth day.*—The pain in the shoulders had re-

turned, being most marked in the left shoulder; so that when the animal was led about, he rested the left fore-leg entirely, and moved on the remaining three. He was exceedingly thirsty, and the skin was hot. The first sound of the heart was again obscure, the second strongly accented. The alvine evacuation was still very copious, and manifested the same acid reaction as before. The animal took his food freely. An ounce of the acid was added to his drink.

“Seventh day.—The pain in the limbs had passed away, and he moved about on all four legs. There was no pain of the peritoneum, but the small opening through which the fluid was last injected was unhealed, and had a fistulous appearance, so that a probe could easily be passed into the cavity. The first sound of the heart was lost; the second sound seemed to me intensely accented; but Dr. Halford, who examined the heart on this occasion, thought that the intensity of the second sound was simply relative; that is to say, arising from the loss of the first sound. The animal passed urine freely; the fluid was intensely acid. Through the fistulous opening in the abdominal wall a full ounce of the solution of lactic acid was injected without difficulty.

“Eighth day.—The pain in the left shoulder returned; the animal moved again upon three legs. The first sound of the heart was now heard as a soft, blowing murmur; the second sound was sharp, as before. Another ounce of lactic acid was thrown into the peritoneum; but some returned at the time, and most of the remainder afterwards.

"Ninth day.—He was better, and free from pain in the limbs. The heart-sounds were clearer, but not well defined. The urine was neutral; there was no purgation. On probing the fistulous opening, it was found closed at the lower part. A small opening was, therefore, made on the opposite side, and two ounces of the acid, diluted with the same quantity of water, were injected.

"Tenth day.—The animal was dull, and very thirsty. The action of the heart was quick and tumultuous, but both sounds could be distinguished. There was slight hacking cough.

"Eleventh day.—The animal was in the same state; he passed large quantities of strongly acid urine. There was free alvine evacuation. The dry, hacking cough continued, and there was great indication of pain over the heart; the heart's action was quick and irritable.

"Twelfth day.—The symptoms were generally the same; the urine was copious, and strongly acid. There was great pain excited when the stethoscope was placed over the heart, and the heart-beats were still quick and irritable. The first sound was again obscure, and the short cough was constant; but the respiratory murmur was clear. The alvine excretion was free.

"Thirteenth day.—The symptoms were much aggravated; the application of the stethoscope produced great expression of pain. The heart-beats were still quick and irritable; the first sound was obscure, the second accented. The animal vomited several times to-day, and refused food; but drank water freely. The

urine in the morning was strongly acid; at night neutral. In the evening he was so snarlish, and winced so much when the stethoscope was applied to the chest, or when the joints of the shoulder were moved, that only an imperfect exploration of the heart could be made. The first sound could not be detected, and there was great irregularity of action.

“Fourteenth day.—The general symptoms were the same. The urine was copious and intensely acid. The short cough continued. There was much thirst, and occasional vomiting of a little mucus. There was still considerable pain over the region of the heart, and a loud and pure systolic bruit was now heard. In the evening Dr. Cockle examined the heart, and confirmed the diagnosis of the systolic bruit.

“Fifteenth day.—The general symptoms were the same. The cough continued. I noticed, for the first time, a little fulness of the abdomen. Drs. Fuller, Cockle, and Meredith saw the animal this day, and listened to the systolic murmur, which was extremely well marked, and could be heard generally in the cardiac region. In the course of the day the tension of the abdomen increased rapidly, showing that there was some effusion; and in the evening I drew off, by puncture, six ounces of thin, darkish fluid.

“Sixteenth day.—The general symptoms were the same. The systolic bruit was very loud and harsh. There was a little exudation from the wound. The urine was strongly acid. Drs. Hawksley, Webb, and Cockle examined the heart this day, and confirmed the diagnosis of a systolic bruit.

“Seventeenth day.—On visiting the animal this morning, I found a return of the pain in the limbs, together with considerable exhaustion. When his limbs were touched he grew angry, but could not stand or move his head without great pain and difficulty. When food and drink were brought, and placed conveniently for his mouth, he raised himself slowly, so as to eat and drink, which he did with great avidity. During the whole of the day he could not move his limbs, and cried when they were touched. The systolic bruit was still well marked.

“Eighteenth day.—Early on this morning the tenderness in the limbs entirely disappeared. He ran about freely, and took his food and drink as though in perfect health. At ten o'clock A.M., Dr. Todd saw him, and confirmed the fact of the systolic bruit; there was, however, some little tenderness over the region of the heart when the stethoscope was applied. A few hours later he suddenly relapsed into his previous condition, becoming violently enraged whenever his limbs were touched. He vomited a little once, and was freely purged two or three times, the excretion having a neutral reaction; but he passed a large quantity of strongly acid urine. After this he seemed greatly relieved.

“Nineteenth day.—He was more lively, and took his food heartily. He was frequently purged, the excretion having a yeast-like appearance and acid reaction. The urine had a strong acid reaction. The systolic bruit was loud and prolonged. The body of the animal had, on this and several days past, a

peculiar sour smell, which scented the whole of a room very quickly.

“*Twentieth day.*—The animal continued the same. There was free purgation; the excretion still had an acid reaction.

“*Twenty-first day.*—He was better. The purging had ceased altogether. He ate and drank well, and ran about without difficulty; but he gave evidence of pain when the stethoscope was firmly placed on the chest. The systolic bruit was very loud, and was heard over a large surface.

“From this time recovery rapidly took place, and day by day the systolic murmur became more feeble, and the pure first sound more obvious. After three days, the murmur being now almost extinct, I narcotised the animal to death with the fumes of the *Lycoperdon Gigantum*, and opened his body in the presence of several of the gentlemen who had seen him during life. All that remained of the endocardial lesion, was a series of fibrinous beads along the border of the mitral valve; the beads were firm and pearl-like. One of the aortic valves was slightly thickened, and quite opaque. The joints showed no pathological evidence of the disease to which they had been subject” (p. 385).

These very remarkable experiments, and the inferences drawn from them by this careful and cautious author, suggest to us two questions of great practical importance. The first is—Assuming it proved that the proximate cause of acute rheumatism consists in an excess of lactic acid in the blood, and supposing

it ascertained that, by assisting the discharge of this through the natural emunctories, such as the skin and the kidneys, can we combine what is called the eliminative system of medicine with the Homœopathic or specific, without injury to the latter? The second question is—Do the Homœopathic remedies which cure acute rheumatism, do so by virtue of their power to excite an excessive generation of lactic acid, or some analogous so-called *materies morbi*?

There can be no doubt that the eliminative method is far the most rational and most successful of all the plans proposed for the treatment of acute rheumatism, except the Homœopathic. It consists in administering effervescent draughts composed of Citrate of Potash or Ammonia, with an excess of from forty to fifty grains of the Bicarbonate of Potash. In regard to the success of the treatment of acute rheumatism by the alkaline method of treatment, Dr. Fuller states, that out of 168 cases, the pain was relieved, and the inflammation of the joints greatly lessened, within twenty-four hours in 54 cases; within forty-eight hours in 56 other cases; within three days in 15 cases; within four days in 9 cases; and within five days in 3 cases. The articular inflammation subsided altogether within twenty-four hours in 7 instances; within three days in 33; within four days in 20; within five days in 13; and within seven days in 14; while, in the remaining 24 instances, although it did not wholly disappear until a later period, yet its activity was subdued by the fourth day (p. 136). It is but fair to observe, that besides the use of the alkaline

draughts, Colchicum and Opium were also given, but that the main benefit is attributed to the alkalies. So that, while on one hand we find acute rheumatism caused by lactic acid, we observe, on the other hand, that this disease is subdued by the neutralisation and elimination of that poison from the system. What is there to forbid us from employing the same or similar means? The reply that starts to the lips of a disciple of Hahnemann is, that to do so would be to prevent the curative action of his remedies. Is this the case? At first sight, doubtless, it seems absurd to expect that the ten-thousandth part of a drop of the tincture of Aconite or Bryonia could exercise any influence upon a patient who was taking ounces of potash. But is this more incredible than that a fraction of a grain of common salt, or of lime, or of sulphur, should act upon a person who was consuming many million times this quantity of these substances at every meal? Certainly not; and here we must dismiss all *à priori* reasoning, and be guided simply by experience and experiment. Some of our most scientific colleagues are in the habit of giving alkaline drinks along with the appropriate Homœopathic remedies, and the results are said to be highly satisfactory. I have never done so in this hospital, because I have never met with a case in which they were indicated, which did not yield rapidly to the Homœopathic remedies; and I think it would be a mistake to complicate the results obtained here by the admission of any auxiliaries that were not absolutely necessary. But, after a careful consideration of all

the facts, and of the objections that may be urged against the use of some forms of alkaline drink, I am free to confess that I think the employment of such means perfectly admissible, and that, in this instance at least, there is no objection to a combination of the eliminative with the Homœopathic system of medication.*

The second question is—Do the Homœopathic medicines cure acute rheumatism by exciting an excessive generation of lactic or some analogous acid? The two medicines we generally find successful in the treatment of this disease, seem to possess the power of developing acid in the system. Among the symptoms of Bryonia, recorded in Hahnemann's proving, is (762), "copious, *sour-smelling* sweat during a good night's rest." Among those of Aconite, published by the Austrian Society for proving medicines, we find (768), "sour-smelling sweat all over the body;" and Stoerck, an old writer on Aconite, makes the curious observation, that Aconite is of especial use when the *materies peccans* is to be removed by transpiration. If we compare the effects of Aconite, as described in the Austrian account just referred to, with those of Lactic Acid, as given by Dr. Richardson, the resemblance is striking. Here is a group produced on a

* I am informed by Dr. Porges, of Carlsbad, that he finds the Homœopathic medicines, even in the highest dilutions, produce their full effect upon patients who are drinking their daily allowance of the Carlsbad waters; nay, he maintains that, so far from lessening the sensibility of the system to the action of infinitesimal doses, the waters seem to have the very opposite effect.

medical student, who took fifty drops of the mixture of Aconite for thirteen days:—"Tearing pain in the arms and legs, and in the knees and elbows, for several days; then cessation of these pains, and violent palpitation of the heart in their stead. Two days afterwards the tearing pains in the limbs recurred, and the palpitation ceased. The next two days the palpitation recurred, with feeling of tightness in the chest, the limbs keeping free of pain." If a physician had been called in to see the subject of this experiment, would he have had any hesitation in pronouncing it to be a case of acute rheumatism? On the other hand, if a physiologist had been told that the effects were those of a poison, would he not have at once suggested lactic acid as the noxious agent? I do not mean to affirm that Aconite and Bryonia cure acute rheumatism by exciting the formation of lactic acid in the blood. We have not yet sufficient data to warrant this inference. I merely wish to point out, that we should direct our observations to this point, for it is one of invaluable importance to the future development of our therapeutic system. What all thoughtful practitioners of Homœopathy lament, is the vagueness of the indications for the selection of the remedial specific. The only way in which this uncertainty can be radically removed, is by ascertaining, what may be called, the cardinal relations between the effects of medicines and the symptoms of disease; in other words, the discovery of the proximate cause of spontaneous morbid action, and of substances which have

the power of exciting some such proximate cause. For example, there are very many medicines which correspond in the symptoms they excite with those of acute rheumatism, if we use this term to represent articular and muscular pain; but when, to *the pains* of rheumatism, we add a high pulse and *copious acid perspiration*, then we find our choice at once narrowed to a very small number of remedies, and in this little group we find the medicines which experience has taught us to be the best for the purpose of cure. Thus will physiology, pathology, and therapeutics walk hand-in-hand, each throwing light upon the other; and those who employ the key left us by Hahnemann, will be at once able to take advantage of the labours (otherwise comparatively barren of practical results) of the eminent physiologists and pathologists of our day.

Besides affording us positive, constant, and exact indications for the identification of the appropriate medicines for acute rheumatism, which is proved to depend upon the presence of an acid in the blood, it may enable us to employ, with benefit, medicines which have a power of controlling the formation, or assisting the removal, of the noxious acid which is often the cause of a multitude of anomalous symptoms in persons of a gouty or rheumatic habit. For example, I lately attended a lady of a gouty diathesis, who, for some weeks, had lost her usually good appetite. She had become extremely irritable; occasionally the mental irritability passed entirely beyond her own control, and she became, for the time, delirious. This

was generally of an evening. The delirium was not attended with a high pulse, but with great prostration and various hysterical symptoms. One constant condition, both when in health and during her late attack, was a nearly total incapacity of sensible perspiration. Even in the hottest summer weather she never knew what it was to be moistened with perspiration. After about a fortnight's treatment, during which time there was a steady increase of all the morbid symptoms, she was suddenly covered, from head to foot, with a very copious, sour-smelling perspiration, and at once experienced relief. Acting on nature's hint, I ordered a lamp bath, and from that time, under the use of Bryonia, she steadily improved. In this case I have little doubt that there was an excessive formation and deficient elimination of lactic acid; and had I to treat it over again, I should be disposed to give Aconite at the commencement. It is true that there is an apparent deviation from the first principles of Hahnemann, if we substitute the pursuit of the proximate cause for the general resemblance of the symptoms of a disease with those of its corresponding specific. The difference, however, is not really essential; and this method does not come under his just condemnation of treating imaginary causes, instead of being guided by manifest symptoms. To ignore a proximate cause of acute rheumatism, when we are aware of its existence, would be as unwise as to make no account of the condition of the lung in latent pneumonia.

But another objection will naturally occur to the inquirer, and take the form of such a question as this

—How can it be that, if lactic acid be the proximate cause of acute rheumatism, the administration of a substance which increases this noxious element will cure the disease? Here we have to do with an actual poison; and surely we cannot suppose that, by addition, the poison will be neutralised. The question drives us back to the root of the whole matter, and we must ascertain how this poison is generated.

According to Simon,* the amount of fibrine in the blood is always in inverse proportion to that of the blood-globules. In acute rheumatism, *so long as the fever lasts*, the quantity of fibrine is greatly increased, and the blood-globules are correspondingly diminished. What has become of these blood-globules? It would seem that they had undergone a process of decomposition; and one of the products of this change is lactic acid. Thus acute rheumatism may, perhaps, be described as a primary affection of the blood-globules, which terminates in their death; and the symptoms of the disease are due to a twofold cause—first, the absence of the proper allowance of these highly organised floating reservoirs of nutrition; and, second, the presence of the substances to which their death and decomposition have given rise. The eliminative system of medication consists in affording a decent removal of this dead, offending matter. Can more be done? Can we arrest the changes in the blood, prevent its decomposition, and, consequently, the formation of the poisonous agent, which constitute the proximate cause of the disease? “If,” writes Dr.

* Simon's *Chemistry*, p. 153.

Richardson, "I venture to speak of inflammatory blood, as blood in which the oxidation process is abnormally active, or let fall the suggestion that in this state the iron enclosed in the blood may possibly have become the magnetic oxide, and that the rapid aggregation of these cells in inflammatory blood, has thus some relation to the magnetic force, I do so, open to the correction of more extended observation." If this or any analogous hypothesis be verified by future experiments, it will supply the missing link between the action of infinitesimal doses of medicines, and the resultant chemical changes in the body. Without identifying electricity and nervous force, we may safely affirm that these two are closely allied, and constantly act the one upon the other. There is, probably, no vital movement without a corresponding change in the electric condition of the moving parts, and all vital movement is exerted by some action of the nervous system. To command the nervous system is to possess the power of regulating all vital and all chemical actions in the body. It is this that gives such enormous power to our medicines. The dose of Aconite or of Bryonia we administer may act on the pneumogastric nerves, and influence the conditions of the formation of the blood-globules at the lungs—may preside, so to speak, over their nativity. The *modus operandi* of their efficacy in acute rheumatism may be that, by exciting in them an over-quick life, they produce a premature and partial disorganisation, which results in the development of lactic acid; and that, by this early and mild discharge, they exhaust the suscepti-

bility of the blood-globules towards the natural exciting causes of the changes which lead to large and deleterious formation of this substance in the blood. If the proposed explanation of the action of our medicines be correct, we have a repetition of the process by which immunity is afforded against small-pox by vaccination. By the administration of Aconite or Bryonia, we anticipate a natural acute rheumatic attack; and by anticipation we cure it—that is, we force it to rapid and exhaustive development.

How far the artificial introduction of lactic acid into the blood, by the administration of whey for drink, may antagonise its natural formation in morbid excess, is an important practical question, and one of curiously historical interest. It is the fashion to represent Sydenham as the patron of heroic practice, and his large bleedings in acute rheumatism are constantly referred to. But, as I have elsewhere* attempted to show, this is an unjust representation of our English Hippocrates. Sydenham, it is true, employed depletion, but he did it under protest; and the more experience he gained, the more he turned from this measure to simpler and safer means. Dr. Barlow thus writes in the *Cyclopædia of Practical Medicine*:—"One of the earliest practical writers on this complaint was Sydenham; and so direct and judicious was his practice, that we cannot do better than make it the text on which to comment in treating this part of the subject." Dr. Barlow then quotes a passage from Sydenham's works, beginning with the following

* *History and Heroes of the Art of Medicine.*

paragraph:—"Since both kinds of this disease seem to arise from inflammation, as appears from the concomitants just mentioned, and especially by the colour of the blood taken away, which exactly resembles that of persons in pleurisy—which is universally allowed to be an inflammatory disease—so I judge that the cure ought to be attempted only by bleeding, the heat of the body being in the meantime abated by cooling and thickening remedies, along with a proper regimen." Sydenham published this in the year 1676. Subsequent experience, however, induced him to modify his practice; and the latest record of his views in regard to the treatment of acute rheumatism, is contained in a letter addressed, in the year 1679, to Dr. Brady, to the following effect:—"Respecting the treatment of rheumatism, concerning which you put some questions, I, like yourself, have lamented that it cannot be cured without great and repeated losses of blood. This weakens the patient at the time, and, if he have been previously weak, makes him more liable to *other* diseases for some years. Then the matter *that created the rheumatism* falls upon the lungs, in case the patient take cold, or from any other slight cause. By this the latent disposition exhibits itself in act and deed. For these reasons I determined to try whether any other method besides that of repeated bleedings would cure the disease. Reflecting upon this, and arguing that the disease arose from inflammation—a fact of which, one proof, out of many, is the pleuritic character of the blood—I judged it likely that diet, simple, cool, and nutritious,

might do the work of repeated bleedings, and save the discomforts arising therefrom. *Hence I gave my patients whey instead of bleeding them.*" Sydenham seems to have been satisfied with the result of this innovation. It is certainly a remarkable fact that he should have successfully treated a disease, the proximate cause of which is an excess of lactic acid in the blood, by the administration of a drink which contains lactic acid as one of its most important ingredients. I have ordered whey to some patients who were suffering from acute rheumatism, and the cases went on perfectly satisfactorily. However, I do not mean to build much either upon Sydenham's practice, or my own few trials; I only wish to direct your attention to the facts, as interesting and curious. Besides the points I have touched, there are many others which it would be profitable to discuss in regard to the preference of Aconite to Bryonia, or *vice versâ*; as well as whether these two medicines should be given in succession or in alternation; or whether either by itself is sufficient to arrest acute rheumatism; or whether other medicines, and what other, are required—to say nothing of the vexed question of the proper dose. All this, however, must be postponed for the present, to be, I trust, considered on a future occasion.

LECTURE II.

ON AFFECTIONS OF THE HEART IN CONNECTION WITH ACUTE RHEUMATISM.

"IT is startling, on the retrospect of a large hospital practice, to observe how many hard-working men and women perish miserably, before their prime, from disorders of the pulse and breath, consequent on attacks of acute rheumatism." This general remark of a practical writer, Dr. Wilson, is borne out by the statistics collected by Dr. Fuller. At page 264 of his work, he gives us a table, showing the proportion of cases of acute rheumatism in which the heart was observed to have been affected with some form of disease. The result there exhibited is, that, out of 588 cases of rheumatism, there were 306 of organic affection of the heart. In almost every case of severe rheumatic fever which has been under my care in this hospital, the heart was more or less affected. The case I am about to relate, however, presented this peculiarity, that in it the rheumatism fell, with its full force, first upon the heart, and produced a series of symptoms of the most dreadful and dangerous character. For some days, indeed, very little hope was entertained of the patient's recovery.

J. T—, aged 24, a painter, was admitted on the 22nd of September, 1859. He states that a year ago he had an attack of acute rheumatism, and that, towards its termination, he was seized with intense

pain in the region of the heart, accompanied with oppression of the breathing, so that he could not lie down for some time; that, although so far recovered as to resume his work, yet he has, since that time, suffered more or less from difficulty of breathing, and frequent attacks of cough. This was his condition six days ago, when he was suddenly seized with intense pain shooting through the chest, from the back to the front, attended with great prostration and loss of appetite. At present he looks exceedingly anxious and distressed; complains of great pain about the epigastric region, increased by coughing, and pain in the left arm. There is a short, dry cough, and dyspnoea; the pulse is 104, sharp and small. On examination of the chest, the impulse of the heart was felt below the sixth rib; there was a strong bellows-murmur, accompanying both sounds of the heart. The tongue was white and coated; the urine dark and scanty. There was slight perspiration.

R. Aconite, 1st dil. A drop every three hours.

23rd.—No change.

Continue medicine.

24th.—Complains much of pain in the region of the heart.

Spigelia, 3rd dil. A drop every two hours.

26th.—During the night of the 24th there was sudden aggravation of all the heart symptoms; the cough was almost incessant, and very hard, and the

difficulty of breathing so great that he had to be propped up in bed, and seemed in danger of instant death. For this he got Lachesis, 5th dil., a drop every two hours, which was continued during the 25th. At present (26th, 10 A.M.) he complains of dreadful pain a little below the centre of the sternum, with much oppression of the breathing.

26th, 2 P.M.—Pulse 92, sharp; bellows-murmur strong; very great difficulty of breathing, with frequent, hacking cough, and great restlessness. He cannot lie down, and is quite unable to sleep.

R. Aconite, 1st dil.

Bryonia, 1st dil.

A drop of each alternately every hour.

27th.—Passed a better night, although he hardly slept; he was less restless, and did not suffer so much from attacks of suffocation. *Copious perspiration* during the night; pulse 84; breathing much easier. The pain is now felt directly over the heart. On applying the stethoscope, a distinct, rubbing sound is detected about the fifth rib; the valvular murmur is fainter; the urine is thick, and, on being tested, was found to contain a large quantity of albumen.

Continue Aconite and Bryonia.

28th.—Pulse 84, full and sharp; complains of severe pain over the apex of the heart.

Continue the same medicine.

29th.—Passed a better night; had more sleep; cough very troublesome, and attended with much pain about the region of the heart; pulse 78; rubbing sound still distinct; considerable œdema of both legs as high as the knees.

R. Mercur. Sol., 1st trit. A grain every hour.

30th.—The pulse having reached 84 last night, the Aconite was repeated; after that he slept for about two hours, in a recumbent posture, which he had not since the day of admission, but woke with violent pain at the heart, and sense of suffocation; pulse 78; friction-sound less; bruit very distinct; cough frequent and painful.

R. Tart. Emetic, 1st trit. Every hour.

October 1st.—Passed a better night; more sleep; less suffocation and restlessness; pulse 80; both friction-sound and valvular murmur less marked. The urine, on examination, was found to be free from albumen; it is very copious, and loaded with lithates.

Continue Tart. Emetic and Aconite alternately.

2nd.—Pulse 78; appearance greatly improved; complains of intense pain at the heart; neither the rubbing sound nor the valvular murmur are so distinct.

R. Spigelia, 1st dil. A drop every two hours.

3rd.—Pulse 84; less pain at heart; no friction-

sound audible; still valvular murmur; cough in paroxysms; less œdema; pain in abdominal muscles.

Naja Trip., 3rd dil. A drop every three hours.

4th.—Pulse 104; bruit louder; less muscular pain.

R. Acon., 1st dil.

Bryon., 1st dil.

Alternately every two hours.

5th.—Pulse 82; no friction-sounds, œdema nearly gone.

Continue medicine.

6th.—Pulse 80; cough much better. Complains much of pain extending from the region of the heart through to the right side of the chest.

R. Acon., 1st dil.

Arsen., 4th dil.

A drop alternately every four hours.

7th.—Pulse 80, smaller; no cough; loud valvular murmur; less pain in chest.

Continue medicine.

9th.—Pulse 80. Complains of much pain in the left shoulder.

Continue medicine.

11th.—Passed a much better night. The bruit over mitral valve is very loud; much throbbing of carotid and temporal arteries.

Continue Aconite, and
Naja, 3rd dil.

A drop of each alternately every four hours.

13th.—Less pain in region of heart; no cough; some pain of shoulders.

Continue medicine.

19th.—Is now entirely free from all pain; sleeps well; appetite good. The bruit continues, but in other respects he is well.

Continue Naja Trip.

25th.—Discharged.

This report conveys a very faint impression of the severity of the case. In fact, it is impossible for words to depict the expression of mortal anguish of this man's face, caused by a sense of impending suffocation. For several days he was so ill that his death was hourly expected, and there was almost no hope entertained of his recovery by all who had an opportunity of observing him. Although we class it as rheumatism, it was really a case of severe endocarditis, complicated with pericarditis; and it affords an excellent text for our observations upon this common and often deadly complication of acute rheumatism.

The first difficulty we encounter in pronouncing an opinion upon such a case as this, consists in our ignorance of the condition of the heart *before* the attack. When this man was admitted he was un-

doubtedly suffering from inflammation of the lining membrane of the heart, and of the valves of the heart. But what was the state of the valves? Were they in their normal condition, or was this a *disease* of a *diseased* heart? In this instance there can be no doubt, from the history of the case, that the heart had sustained organic mischief in the previous attack of acute rheumatism, from which it had never recovered. The symptoms he suffered from during the interval between his first and second attack, and the enlargement of the heart, place this beyond a doubt. Frequently, however, it is impossible to say whether a valvular murmur, in a case of acute rheumatism, be of old standing, or have arisen in the course of the attack for which the patient is being treated.

Before commenting upon the treatment of endo- or pericarditis in connexion with rheumatism, let us direct our attention to the pathology of the disease. The doctrine of metastasis, in regard to endocarditis and acute rheumatism, is now generally abandoned. The more severe the pain and inflammation of the joints, the more, and not the less, have we to dread the participation of the heart in the morbid action. In the case before us, however, there was scarcely any general articular or muscular pain; and the whole of the inflammatory action was confined to the heart *during this attack*. The same patient was re-admitted some weeks afterwards, and then we had the converse of the phenomena. The case is of sufficient interest to deserve quotation.

The same patient who was dismissed on the 25th of

October was re-admitted on the 5th of December, and the following report was made :—

He complains of severe pain in the left thigh and right leg, extending from the knee to the foot. Last night he had much pain in the abdominal muscles, chiefly over the pubic and iliac regions. He complains also of a sharp pain between the shoulders, extending forwards to the left side ; this pain is much aggravated by eructation or coughing, and seems to be an affection of the diaphragm. The action of the heart is strong and irregular ; both sounds are accompanied with a loud, grating murmur. *There is no pain in the region of the heart.* The pulse is 108, full, bounding, with a peculiar thrill. Although he does not suffer pain in the heart, he complains of an unpleasant sensation, as if it stopped for a moment and then went on with a bound that sent the blood with painful force through the head, and affected his sight. No difficulty of breathing ; he can assume the recumbent posture without distress. A frequent hacking cough ; pain across the upper part of the chest, aggravated by turning round the body ; nausea, and occasional vomiting ; sleeps very badly.

R. Aconite, 2nd dil.

7th.—Complains of great pain in abdominal muscles.

R. Bryonia, 2nd dil.

9th.—No pain in heart ; no difficulty of breathing. Severe pain still in the muscles of the abdomen. Less pain in legs.

Continue medicine.

12th.—No change ; still much pain. No change in the action of the heart.

R. Rhus Toxic., 3rd dil. A drop four times a day.

14th.—Better ; less pain.

Continue medicine.

19th.—Much better ; pain almost entirely gone.

From this time he steadily improved, and was dismissed on the 24th, free of all pain. There was, however, no amelioration in the condition of the heart.

The most interesting feature of this case is, that notwithstanding the diseased state of the heart, and the presence of rheumatic action in its immediate neighbourhood—for the symptoms pointed to the abdomen and diaphragm as the chief seats of the last attack—yet the old inflammatory action of the heart, of which he had so nearly died, was not rekindled.

To return to the pathology of endocarditis. Whether this depends upon the presence of lactic acid in the blood may not be positively ascertained ; but it is certain that the disease is accompanied by a morbid condition of that fluid ; and most probably the inflammation of the heart is owing to the action of the blood upon it. The blood is not properly regenerated at the lungs ; it enters the left auricle loaded with fibrine, and probably with lactic acid ; the presence of the latter excites inflammation of the lining membrane of the heart, while the excess of fibrine is ready to be deposited on the free edges of the valves, and

to give rise to temporary or permanent derangement of their mechanism.

Whether the morbid phenomena elicited in the experiments of Dr. Richardson with lactic acid were those of actual rheumatism, or only bore a close resemblance to that disease, they are, at all events, highly instructive; and probably the series of changes observed in the valves are nearly identical with those which it induces.

At page 373, he narrates the following experiment:—“He injected seven drachms of lactic acid into the peritoneum of a cat. During the first hour after the operation the animal breathed at the rate of forty-five times in the minute; the surface of the body was cold, and she was restless. Two hours later the respirations were forty-two per minute, and *the heart beat so irregular and irritable* that it could not be counted accurately. About six hours after the operation, she was left, in the same condition, for the night, being provided with a straw bed and food. In the morning she was found dead, and with the body in a state of intense rigidity. On making a *post-mortem* examination, while the other viscera were found in their natural state, except that the lungs were somewhat too red, the heart presented the following appearances:—The right side was healthy. The left auricle contained a small coloured coagulum, which interlaced feebly with the muscular structure of the wall. The left ventricle contained some fluid blood. The mitral valve was greatly *thickened, red, and cedematous*. On the surface of the valve there was a small fibrinous

deposit, which adhered to its position by means of an under-layer of a white, glutinous fluid, like that which could be made to exude from the valve on puncturing it with a needle. The surface of the endocardium lining the left ventricle had an intensely vascular appearance, resembling, in fact, bright-red velvet. The pericardium was dry, and slightly injected. There was no trace of mischief in the joints, and the brain was normal."

Here we have an admirable description of the first stage of endocarditis. When the heart is examined at this period, the first sound is heard as muffled, or is absent altogether; the second sound preternaturally sharp and loud. There is also a short, irritable cough, not dependent upon any mischief in the lungs, but on the condition of the heart. On the detection of this stage, and on his power to arrest it, depends the practitioner's capacity of saving the life of his patient; for if the fibrinous deposit adhering to the edges of the mitral valve by the glutinous fluid should remain there, it would, before long, become organised; the valve would be unable to close accurately; at each contraction of the ventricle there would be a certain regurgitation of blood into the left auricle, oppressing the lungs on the one hand, and, on the other hand, by demanding excessive action of the ventricle to perform its immoderate task, giving rise to hypertrophy of the heart; and the inevitable sequence of painful and distressing symptoms, which make life hardly endurable to the sufferer, would follow. On the successful treatment of the first stage of endocarditis, depends the

future bodily welfare of the patient. Are we in a position to cope with this successfully? Are we warranted in rejecting the measures employed against it by the vast majority of the medical profession?

What are these measures? The first, in point of importance, is blood-letting. What is likely to be the effect of blood-letting in reference to the deposition of fibrine upon the edges of the valves and its organisation? It is an admitted fact, that the greater the proportion of fibrine in the blood, the more liable it is to be effused and to form deposits. Now, it is stated by Simon, in his great work on *Animal Chemistry*,* that the amount of fibrine is always in inverse ratio to the amount of blood-globules; and it has been proved by Andral and Gavarret, that the quantity of blood-globules decreases directly as blood is withdrawn by venesection, and in exact proportion to the amount and frequency of the operation. So that large and repeated blood-lettings tend to produce the precise condition most favourable to the formation of the great source of danger in acute rheumatism. It need not, then, cost us much distress to dismiss the lancet in *this* disease, at all events.

The other means most in repute are the administration of Alkalies, Opium, and Colchicum. Of the first I have spoken in a former lecture. All it professes to do is to neutralise the exciting cause, not to cure the disease excited; the inflammation of the heart is not cured by Alkalies, but the heart is allowed to recover. Opium and Colchicum, especially the latter, by acting

* P. 158.

specifically upon the heart, may, indeed, be positively curative agents; but they are, undoubtedly, inferior to Aconite and Bryonia. From a pretty long experience of the action of these remedies in acute rheumatism, from having observed, in frequent instances, the rapid disappearance of the endocardial murmur under their use, I have no doubt whatever that in them we have remedies which possess the double power of, in some way or other, arresting, neutralising, or eliminating the exciting cause of rheumatic fever, and quenching the incipient inflammation of the heart. I do not mean to confine this power to these two medicines; but, so far as my observation extends, these two are the most frequently indicated, and, when indicated, are almost certainly effective.

It thus happens, that however formidable in its consequences the complication of incipient endocarditis may be, it does not, practically, render the treatment of acute rheumatism more difficult to those who employ the Homœopathic method of treating this disease. But when the first stage has been allowed to pass, and instead of simple redness of the lining membrane of the heart, and œdema of the valves, there is permanent thickening of their edges, and contraction of the tendinous cords by bands of organised lymph which bind them down, then the obstacles to successful treatment are much increased by the perplexity of the problem presented to the practitioner. The first difficulty is knowing what the case really is. In the one before us, the symptoms all pointed to the heart as the seat of disease. Was this a case of simple endocarditis, or of

rheumatic endocarditis? The reason for making the distinction is, that if the inflammation of the heart be excited by an abnormal state of the blood, so long as this condition continues we can hardly expect to arrest the cardiac disease; if, on the other hand, the inflammation of the heart be simple and non-rheumatic, we may select remedies which act upon that organ, irrespective of their relation to rheumatism and its cause.

In the case before us we could have no doubt of the rheumatic character of the inflammation of the heart, and the patient got a drop of the first dilution of the tincture of Aconite every three hours for two days. At the end of that period he complained very much of the pain at his heart. This, indeed, was his only complaint. The pulse—in uncomplicated, acute rheumatism, the invaluable test of the progress for better or worse—afforded less than its ordinary assistance here, on account of the permanently abnormal condition of the circulation of the blood; and this is one of the embarrassments always met with in the management of this class of complicated disorders—that is, of *diseases* of a *diseased* heart. The medicine then prescribed was Spigelia. Among the symptoms of this medicine, recorded by Hahneman, are the following:—

“231. Dull, repeated stabs, when the impulse of the heart is felt, only more external, synchronous with the pulse.

“232. Dull stabs at the place where the heart is felt to beat.

“234. Unusually strong beating of the heart, so

that he not unfrequently hears its pulsation, even through the dress; it could also be observed beating.

“335. Palpitation of the heart, and anxious oppression of the chest.

“337. Increase of the palpitation on sitting up and bending the chest forward.

“339. When he sat down, early in the morning, on the first rising from bed, the heart began to beat strongly, and, over the spot where the beating was felt, there seemed to be a heavy painful pressure, as of a weight, which caused oppression of the breathing. Accompanying this was felt cutting and boring, as from incarcerated flatus in the lower intestines, which outlasted the palpitation of the heart.”

We are not informed of the doses taken by the prover of this medicine; but, as Hahnemann, in its introduction, mentions the effects of sixty, eighty, and one hundred drops of the tincture, we may assume that the experiments were made with large as well as small quantities.

That these symptoms indicate a direct and powerful action on the heart is undeniable; but it is a question, and one of extreme importance, whether *Spigelia* is suitable for rheumatic carditis, or only for idiopathic inflammation of the heart. Certainly the rheumatic are not nearly so prominent as the neuralgic symptoms in the proving of the medicine, nor has it taken its place among the well-accredited remedies in acute rheumatism.

I throw out this suggestion, because, in the few fully-detailed cases of endocarditis which have been

published in the English tongue, Spigelia, although employed, has seldom been given by itself—generally in alternation with Aconite. I have some doubts whether Spigelia be so well suited to rheumatic endocarditis as is generally supposed. At all events, the results in the case before us were negative. The patient took a dose every two hours, from the afternoon of the 24th; but during the night of that day, the cough and distress of breathing increased to such a degree, that the house-surgeon (Dr. Fenton Cameron) was called up, and administered a drop of Lachesis. This medicine gave relief at once, and was continued with advantage during the following day.

That in Lachesis, Naja, and other snake-poisons, we possess such powerful remedies as the effects of the bites of the animals seem to indicate, has been questioned by some of our most experienced and best-informed practitioners. The objection is thus put by Dr. Drysdale* :—

“*First.*—We know that serpent-poison, taken internally in small doses, produces certain symptoms.

“*Second.*—We know that the bite of the same serpents produces certain other and truly *very different* symptoms.

“*Third.*—We know that serpent-poison, given internally in disease, cures certain symptoms.

“I conclude that these last—viz., the symptoms cured—will, by the Homœopathic law, be similar to those in the first paragraph—viz., the symptoms produced by internal exhibition of the poison. But I

* *British Journal of Homœopathy*, vol. xii., p. 264.

hold that at present we have no right to expect that the cured symptoms in the third paragraph should accord at all with the bite-symptoms in the second paragraph. We know that with medicine from the mineral and vegetable kingdom, that remarkable discrepancy in the internal and inoculated symptoms does not exist; and, therefore, till that problem is solved, I hardly think we can avail ourselves of the symptoms of bites of serpents, and stings of bees, as Homœopathic indications for the inward exhibition of poison. But it would be safer to leave those symptoms in the same category as those violent expulsive efforts caused by the over-dose of drugs in the provings, which are justly considered by Hahnemann as little characteristic of the drug, and little adapted for Homœopathic use, and confine ourselves to the indications afforded by the internal exhibition of those substances."

I entirely dissent from this view. I see no proof whatever of any expulsive efforts, on the part of the animal economy, to get rid of snake-poisons, introduced either by the serpent-fang or by the syringe. On the contrary, the poison seems to act as a pure sedative, and depresses vitality. Again, while I admit a difference *in degree* between the effect of the poison injected into the blood and that of the same substance introduced into the stomach, I see no difference *in kind* in regard either to the tissues it affects, or of the nature of the morbid action it excites. The one set of symptoms glides into the other; and the difference is not greater than is to be accounted for by the more

rapid and intense effects of a very large and a very minute dose of any other morbid agent. For example, in the cases of poisoning by serpent-bites, there is scarcely any symptom more constant than a spasmodic action of the muscles of the throat. Thus Mr. Duffin describes the case of a woman bitten in the small of the leg by a cobra di capello. Ten hours after the accident, deglutition was so much impeded that hardly anything could be got to pass into the stomach. In another case of a man, after he had experienced a sense of tightness or tension spreading fast towards the heart, and attended with extremely laborious respiration, he had stricture of the œsophagus to such an extent that nothing could be forced down his throat. If we compare these symptoms with those experienced by Dr. Stewart, Dr. Craig, Mr. Gillow, and others who swallowed a portion of a grain of the poison, we shall find the exact amount of resemblance we should expect from so different a dose of the same poison. Dr. Stewart records irritation, warmth, and rawness in larynx and upper part of trachea, *with short, hoarse cough*, as one of the most marked symptoms he experienced after taking the tenth of a grain of the pure poison. Dr. Craig records "slight pricking in the left side of the pharynx;" and, after another dose, soreness in the left side of the throat much worse. Again, "a sudden attack of fluttering at the heart, with *rising in the throat*." The last symptom was evidently a sense of constriction. Mr. Gillow writes:—"Felt no peculiar sensations until after taking the medicine, when, in about an

hour, I perceived a slight heaviness over the eyes, with *constricted feeling* and *dryness of throat*."

It would occupy too much time to pursue the comparison of the two sets of symptoms; any one who wishes can do it for himself. I prefer directing attention to the marked symptoms which Naja produced upon the action of the heart. Naja and Lachesis act, apparently, primarily upon the nervous system, especially upon the respiratory nerves, the pneumogastric and glossopharyngeal; and for this reason we should expect these medicines to be of service in the irritating, sympathetic cough which attends organic disease of the heart. In the case before us the action of Lachesis was immediate; so that, as a palliative and intercurrent medicine in carditis, it is of the highest value. Besides its action on the nervous system, Naja exerts a marked influence on the condition of the blood. It produces a similar state of that fluid, in regard to its fluidity and the condition of the corpuscles, as is induced by slow poisoning by Antimony, and by the administration of large doses of alkalies. Of the great value of Naja and Lachesis in organic disease of the heart, we are convinced by experience; although, owing to the rapid death which has followed its introduction into the system in all cases which proved fatal, there was not time sufficient for the development of any organic changes in the body.

The indications, then, for the use of Naja and Lachesis, are the presence of an irritating, sympathetic cough, in the acute stage of rheumatic carditis, and afterwards organic change in the valves, giving

rise to tumultuous action of the heart, violent, sudden throbbing, attended with endocardiac murmur and increased size of the organ. It is surprising how rapidly the urgent symptoms, arising from changes which are of a more or less permanent kind, may be subdued by these medicines.

The next medicine prescribed was a preparation of Mercury. The reason for the employment of this remedy in pericarditis, is the almost universal belief, entertained by the most experienced of the old-school practitioners, that it has a direct effect in subduing inflammatory action of serous membranes, and promoting the absorption of effused lymph. "There is no remedy," writes Dr. Fuller,* "of whose curative influence experience and observation have afforded such abundant and unequivocal proofs as of Mercury in active serous inflammation." Again he writes—"I am fully persuaded that, in the acute and sthenic form of pericarditis, nothing is of greater service than the remedy in question." Here, as in the administration of Cinchona Bark in certain forms of ague, the new and old school find a common meeting-ground. It would be folly in us not to employ a direct specific medicine, because its virtues were discovered by experience on the sick, and not by experiments on the healthy. The only question between us and the old school is, the best dose to select. On this point, however, the diversity of opinion is not great between the adherents of Allopathic and Homœopathic Medicines; so that, in re-

* *Fuller on Rheumatism*, p. 233.

gard to Mercury, at least, and some other medicines, the two systems insensibly merge the one into the other; some Allopathic practitioners recommending the tenth of a grain of blue pill, and some Homœopathic administering one grain of the first decimal trituration of *Mercurius Solubilis*. We cannot claim much for the effect of Mercury in the case before us; for as the pulse rose during the night, the Aconite was repeated; and although the patient was able to obtain a little sleep in a recumbent posture for the first time, and the friction-sound was sensibly diminished, yet he was awaked by the violent pain at the heart, and a sense of mortal suffocation. For this condition, Tartar Emetic was administered every hour, and with good results. The dyspnœa abated, as well as the pain, and he was able to lie down, and passed a good night.

The chief indication for the administration of Tartar Emetic was the dyspnœa. If we inquire into the causes of the sense of suffocation in such cases, we find that they are mechanical, chemical, and vital. The blood is arrested in its exit from the lungs, which become gorged; the blood itself is in an unnatural condition, loaded with fibrine, and probably irritating; and last, but not least, the pulmonary branches of the pneumogastric nerve, by sympathy with the cardiac, induce cough and dyspnœa. That these attacks of suffocation are in some measure owing to the state of the nervous system, is shown by the suddenness of their invasion, and equally rapid relief. Now, Tartar Emetic has a powerful action on what I may call inflammatory dyspnœa, such as attends the first stage of

pneumonia ; it also has a powerful action on the condition of the blood, resembling in this the effect of alkalies. "It may seem strange,"* says Dr. Richardson, "at first sight, but it is equally true, that there is a striking analogy as regards pathological effects between the alkalies and the salts of Antimony. I have dissected, with great care, the bodies of animals slowly killed by the alkalies and the salts of Antimony ; and my feeling is, that, in ignorance of the symptoms preceding death, and without the aid of chemistry afterwards, I should feel a difficulty in diagnosing, from bare pathology, between death from Antimony and death from an alkali." Although Tartar Emetic is much more marked in its action upon the lungs than upon the heart, yet the experiments of Sir B. Brodie† seem to indicate that it exerts a direct and specific influence upon the latter organ. Besides, the first effect of the injection of a solution of a salt of Antimony into the circulation, is generally to excite, almost instantaneously, the beats of the heart.‡ We know, too, the alliance between Tartar Emetic and various forms of rheumatism. On all these grounds we should be disposed to expect benefit from the administration of the medicine in the terrible dyspnoea that attends rheumatic endocarditis ; and, so far as my limited experience in its use goes, this remedy has not disappointed me ; and I consider it is worthy of

* *Op. Cit.*, p. 435.

† *Philosophical Transactions* for 1812.

‡ *Vide Wibmer, Die Wirkung der Arzneimittel und Gifte im gesunden thierischen Körper* ; vol. v.

more attention in this disease than it has hitherto received from our school.

After Tartar Emetic had been administered, first by itself, and afterwards in alternation with Aconite for three days, there was marked improvement in the appearance of the patient; greatly diminished dyspnoea and cough; and the physical examination showed that the inflammation both of the pericardium and the endocardiac membrane and valves was rapidly abating. Notwithstanding this general improvement, he complained of intense pain in the heart, and for this *Spigelia* was prescribed. On the following day he was in less suffering than he had experienced since his admission, and we may ascribe this to the *Spigelia*. The intensity of the pains, and their independence of organic change for the worse, seem to indicate that they were rather of a neuralgic character, and for such, doubtless, *Spigelia* is admirably suited. All active cardiac and rheumatic symptoms being now silent, *Naja* was prescribed, with a view to assist in the restoration of the structure of the heart. But the following day (Oct. 4th), a sudden aggravation of the heart symptoms occurred; the pulse rose from 78 to 104, and the endocardiac murmur became much louder. In these circumstances, *Aconite* and *Bryonia* were again prescribed, and apparently with the most decided advantage; for on the following day the pulse had fallen to 82, and œdema of the limbs had nearly disappeared. He took these medicines for two days—that is, to the 6th—when the cough, being better, and the pain at the region of the heart having changed its character, and being described as

extending through to the right side of the chest, Arsenicum was substituted for Bryonia, and continued for the following five days in alternation with Aconite.

The powerful action of Arsenic on the heart and its lining membranes, both internal and external, is thoroughly established, both by pathological anatomy and by the symptoms which have been observed to arise from its administration. In cases of poisoning by Arsenic, according to Orfila, small spots of a bright crimson colour were found in the left ventricle, and principally on the columnæ carneæ. On cutting down on these spots, the colour is found not to be only superficial, but to penetrate the substance of the heart. The right cavities of the heart presented spots of a much deeper red—almost black colour. It would be interesting to compare the effects of Arsenic with those of Lactic Acid upon the heart. The resemblance between the description of the appearance given by Orfila of the action of the former, with the representation of the effects of the latter, displayed by Dr. Richardson, must strike every student of this subject.

Among the recorded symptoms produced by Arsenic, some of the group read exactly like the description of an attack of endocarditis or pericarditis. Thus, a person who had been exposed to the fumes of Arsenious Acid was affected with frequent fainting, tightness in the præcordia, difficult breathing, inextinguishable thirst, parched throat, great restlessness, wakefulness, and pains in the feet. In another case, we find recorded, violent tumultuous action of the heart; pulse 90, strong, and jerking. On the fourth, fifth, and

sixth day, after taking the poison, there was great agitation ; delirium ; pulse 100, full and jerking ; violent and tumultuous beating of the heart, attended with a loud bellows-murmur. Stapf observed in himself, when proving the medicine, excessively troublesome palpitations. When lying on his back, the heart beat both quicker and stronger.

It is not necessary to continue quotations in this direction, as it is admitted to be a well-established fact that Arsenic does act powerfully as a morbid agent upon the heart ; and, on the other hand, its value as a remedial agent in diseases of that organ is universally acknowledged by Homœopathic practitioners. Dr. Black makes the following observations upon the use of Arsenic in affections of the heart ; and his high reputation as a successful practitioner, and trustworthy observer, makes all his practical suggestions worthy of our serious consideration :—"The action of Arsenic upon the heart is very strongly marked, and it holds the first rank in the treatment of cardiac diseases. It is useful in pericarditis and endocarditis, along with Aconite, Bryonia, and Spigelia, and occasionally with Mercurius. It is especially indicated in pericarditis when effusion has taken place. In organic affections, especially when there is dilatation or valvular disease, it is the best remedy." This is too vague and sweeping an observation. To describe Arsenic as the best remedy when there is either dilatation or valvular disease present, is to raise it to a sort of empirical specific in almost all organic affections of the heart. Dr. Black will, I

hope, some day, enter more into detail, and distinguish the stages of the valvular diseases, and the kind of dilatation, whether active or passive, for which he has found it best suited. That is what we now expect from our men of experience—to furnish the means of a differential diagnosis between the class of cases of hypertrophy and valvular diseases of the heart, for which Arsenic is better suited than all other remedies. “It is worthy of note,” continues Dr. Black, “that, under its use, the various pains, the distressing palpitation, the dyspnoea, are relieved in a marked manner, and that, too, while the organic change in the heart remains unchanged.” This is true: at the same time it is surprising how rapidly organic changes do take place in the heart; the variation in the murmurs from day to day, or even from hour to hour, that is perceived in cases of semi-acute endocarditis, must have struck every attentive observer; and, probably, every change in the sound was caused by some alteration in the structure of the heart, whence the sounds proceed; so that it is not impossible that frequently the benefit derived from Arsenic may be owing to its action in the heart itself; although the changes it induces are inappreciable, except in the sympathetic sphere of relations of that great centre of sympathy. That the relief afforded by Arsenic in the numerous functional derangements which attend structural changes in the heart, is due to the action of the medicine upon that organ, is corroborated by the next observation, to the effect, that palpitation, depending on inorganic causes, is not so frequently relieved by

Arsenic as by some other medicines:—"When the palpitations are attended with noise in the ear, rush of blood to the head, or confusion of sight, then," continues Dr. Black, "Lachesis is more indicated than Arsenic." The symptoms here given as being more indicative of Lachesis than those of Arsenic, are those of hypertrophy of the left ventricle; if so, what comes of the statement, that, in dilatation of the heart, Arsenic is *the best* remedy?—unless Dr. Black intends to make a distinction between hypertrophy and dilatation, and confines his remark on the superiority of Arsenic to passive dilatation. This form of dilatation, however, does not co-exist with imperfection of the valves: at all events, imperfection of the valves, as a rule, is attended, not with passive dilatation, but with active hypertrophy.

While I do not think that Dr. Black has been successful in distinguishing the class of cases for which Arsenic is better suited than Lachesis and Naja, I do not propose to attempt any improvement upon what he lays down as the general rule for the treatment of such cases; for it would occupy much more space than we have at our disposal, to enter into a minute detail of all the reasons which, in any given case, should make us prefer the one medicine to the other in the stage of endocarditis for which the two are suitable. In the case before us, after the patient had taken Arsenic for four days, Naja was prescribed, and continued till the time of his discharge, which took place on the 25th of October. He was re-admitted on the 5th of December, having been tolerably well in the

interval. He had been exposed to cold, and had been seized with a fresh attack of acute rheumatism. For this, Aconite, and afterwards Bryonia, were prescribed; but neither of these medicines had much effect upon the pain, which rapidly yielded to *Rhus Toxicodendron*.

He complained most of a sharp pain between the shoulder-blades, extending forward to the left side. The pain was not affected by posture. This closely resembles symptom 573 of *Rhus*—viz., “Severe rheumatic pain between the shoulders, neither relieved nor increased by rest; only mitigated by warmth, and aggravated by cold.” On the whole, however, *Rhus* is certainly not the medicine which one would naturally have selected had the case presented itself for treatment in its existing condition, when the remedy proved so useful. *Rhus* was prescribed chiefly because *Bryonia* did not seem to be producing the effect it had done in the former attack. This opens up a very important question, as to whether medicines lose their power by exhausting the susceptibility of the system to their action—a question nearly related to one which we have postponed for future consideration on the alternation of medicines. Reserving this for a future lecture, let us observe one most interesting and important feature of the second attack in this patient—the absence of fresh mischief at the heart. Can we ascribe this to the action of the remedies prescribed during his former illness? Had the Aconite, *Bryonia*, the *Arsenicum*, and the *Naja* fortified the heart, and enabled it to resist the poisoned state of the blood, which tended to excite disease in that organ? If we

could answer this question in the affirmative, nothing would enhance so much the superiority of Homœopathic or specific treatment over the palliative and depletive. I think it is highly probable, both from analogy and observation, that, in this case, the patient's immunity from heart-disease was owing to his previous treatment; at the same time, there is not sufficient grounds for affirming it positively. One thing, however, is quite certain—that had the patient been largely bled for his first attack, his chances of recovery from the second would have been infinitely diminished.

LECTURE III.

ON RHEUMATISM AFFECTING THE BRAIN.

IN my last lecture our attention was directed to one of the most ordinary complications of acute rheumatism—inflammation of the heart. I propose to begin this one by the narrative of a case in which other organs besides the heart were affected.

Ann Brewer, aged 18, a servant, unmarried, was admitted into the hospital on the 11th of February, 1861. Her mother, who brought her, states that, five years ago, she had a rheumatic fever, which confined her to the house for two months; she only took opening medicine for it. The patient is of a full habit of body, and does not look unhealthy. She states that she was in the enjoyment of her usual good health till a fortnight ago, when she became affected with "sore throat." When still suffering from cold, she went out one damp night, and, on her return, she felt pain, first in one knee, then in her hands and fingers, so that she was compelled to go to her bed. She now complains of intense pain in all her joints, and also in the spine. The pain is much increased by touch, and the slightest movement makes it so much worse that she cannot change her posture in any way. Pulse 120, full. *Heart's sounds perfectly normal.* Skin hot, and covered with perspiration. Tongue dry and cracked; lips dry and brown. She has had one loose motion since her admission.

R. Aconite, 3rd dil., one drop.

Bryonia, 3rd dil., one drop.

A dose of each, alternately, every four hours;
her hands and feet to be wrapped in flannel.

12th.—Covered with profuse perspiration. Passed a restless night. Complains of intense pain.

Continue medicine.

13th.—Pulse 110. Passed a restless night, and complained of the pain being as bad as ever, till the evening, when she reported herself as much better every way. She could move her hands freely, without pain.

Continue medicine.

14th.—Has been violently delirious. Got out of bed several times during the night; there is great difficulty in keeping her in it at all. Talks incoherently; has a stupid look. Passes her motions, which are pale and watery, involuntarily in bed. Skin hot; great thirst,

Continue medicine.

15th.—Pulse 104, regular. Less delirium. Pains in arms and feet much abated. Still frequent loose stools.

R. Rhus Toxicodendron, 2nd dil.

Two drops every four hours.

18th.—Less pain in limbs. No delirium. Much

somnolence. Pulse 120, soft. Complains of pain in cardiac region. On auscultation, slight systolic bruit was heard over the apex of the heart; the sounds of the heart were muffled and indistinct, and a slight friction-sound was distinctly audible. The respiration was very oppressed and hurried—forty-eight times in the minute. The chest was covered with an eruption of sudamina. Frequent liquid stools.

R. Merc. Sol., 2nd trituration.

Two grains every three hours.

20th.—Friction-sound no longer audible. There is a loud double “bruit;” the sounds much more distinct.

Continue Mercurius, and a dose of Aconite, 3rd dilution, at bed-time.

21st.—Pulse 120. Skin hot. Respiration more embarrassed and hurried. Much cough, with frothy expectoration. On auscultation, mucous râles are audible at the front and back of the chest; there is no dulness on percussion. Tongue brown and dry. Bowels still relaxed. Intense prostration of strength.

R. Acon., 3rd dil., one drop.

Pulsatilla, 3rd dil., one drop.

A dose of each, alternately, every three hours.

23rd.—Better. Tongue moister and less brown. Bowels still relaxed. Great somnolence.

Continue medicine.

25th.—Respiration almost normal ; no mucous râles audible. Pulse 120. Less bruit. Tongue moist, and free from crust. Expression of countenance improved. Still frequent, loose stools ; but more under control.

Continue medicines.

March 1st.—General improvement. No purging. Tongue moist and natural. No sweats. Intense prostration.

R. Phosphorus, 4th dil.

A drop every four hours.

9th.—Much better ; can now sit up in bed.

Continue.

11th.—Is now entirely free from all pain, and from all morbid symptoms, with the exception of a slight bruit.

R. Naja Tripudians, 3rd dil.

A drop three times a day.

25th.—Is now quite well ; pulse 80, natural. The action of the heart is perfectly normal, and her strength is rapidly improving.

Independently of the anxiety excited by this case, on account of the imminent and long-continued danger to the life of the patient, it presents features of unusual interest from the variety of the pathological groups it successively presented during its course. To indulge in a metaphor, we might say that the fiend Rheumatism, having entered the body of this poor girl,

assailed in succession every vital organ, and, when driven out, left her in a state of absolute exhaustion, so that her recovery was a matter of general surprise. The fiend entered, as it usually does, under the cover of a cold, damp night ; it made its presence known by pain in the joints ; and, on the admission of its victim into the hospital, there was nothing to indicate anything unusually severe in the assault, except, perhaps, that there was looseness of the bowels, and a brown instead of a white tongue. After being under the usual treatment for two days, she was reported, on the evening of the 13th, to be comparatively free from pain—and that, too, rather suddenly—and there was then every prospect of a speedy recovery. However, that night an entire change took place in the features of the case ; for she became so violently delirious as to require constant restraint to prevent her from leaving her bed and walking about the room. Whence this delirium ? Is this an example of metastasis ? or did it arise from one of the three common causes of delirium ? The *first*, an impoverished state of the system, such as is caused by inanition, by profuse hæmorrhage, or blood-letting, or by some other cause of exhaustion of the vital energies ; or, *second*, the sympathetic action of the brain, with intense inflammation of any important organ, as in the delirium that attends pneumonia ; or, *third*, was the derangement of the cerebral functions caused by the actual presence in the brain of some deleterious substance, such as alcohol or chloroform ?

“At one time,” observes Dr. Fuller, “all cerebral

symptoms which occurred in the course of acute rheumatism were referred to inflammation of the brain or its membranes; resulting, as was supposed, from metastasis of the morbid action to the cerebral structure, in consequence of the subsidence of articular inflammation. But their independence of metastatic action is attested by their not unfrequent occurrence without the subsidence of articular inflammation; and their independence of mischief, developed within the cranium, is abundantly proved by dissection after death, which, even when the case has terminated fatally, fails utterly, in most cases, in affording the slightest evidence of cerebral mischief." In the case of the patient we are now considering, there was a sudden and complete subsidence of pain the evening of the night of her delirium. It may be a question whether this *apathy* arose from a deadened power of sensation, or from a subsidence of the articular inflammation. If from the former, it was merely the first symptom of the cerebro-spinal derangement; if from the latter, it opposes Dr. Fuller's views of the pathology of such cases. We may dismiss the former of these two suppositions in a very few words. The patient showed no symptoms of unconsciousness, and the affected joints were visibly less inflamed. The rheumatism had left *them*, and, for the time, became latent. Dr. Fuller's second position—that because, after death in such cases, no evidence of organic change in the brain or its membranes has hitherto been discovered, therefore (to use his own words) "the independence of the mischief within the cranium is abundantly proved," seems to me untenable.

First, because in the experiments already referred to, made by Dr. Richardson upon dogs, after the most unequivocal evidence of severe articular rheumatic inflammation during life, on examining the carcass after the death of the animal, no trace of organic change in the joints could be detected. And in a case recorded by Chomel, in which he carefully examined the joints of a patient who died while suffering from acute articular rheumatism, that celebrated pathologist was unable to detect, in any one of the joints, the slightest vestige of inflammatory action. If the synovial membrane may suffer from inflammation without structural manifestation, why may not the brain and its membranes? Secondly, do we find evidence of organic change in the encephalon in all cases in which delirium has been present? Do we find it in cases of typhus fever, of delirium tremens, and of various other diseases characterised by the violence and long continuance of this particular symptom? Undoubtedly we do not; and why, then, are we to reject the supposition that, when we meet with delirium in acute rheumatism, the affection arises from a morbid structural, but inscrutable, alteration in the substance or membranes of the brain itself? Let us bear in mind how rapidly structural change is developed and removed in the heart, which would never be detected if we had not the opportunity of a physical exploration of that organ. Had we nothing to guide us but the sensations of the patients, the presence of endocarditis would be unknown in a large proportion of cases in which it exists. In the case of the brain, we can only

conjecture the presence of some change in its mechanism, from the imperfect, or irregular, or violent discharge of its functions.

As there is, at present, a tendency among practical writers to consider the doctrine of metastasis as an antiquated error, at least in reference to acute rheumatism, it may be well to adduce evidence of the undeniable truth that there are forms of this disease in which there is a sudden *transference* of the morbid action from one part to another. The following cases are related by Dr. McLeod in his *Treatise on Rheumatism*, p. 116:—

“A gentleman of literary habits, in the thirty-seventh year of his age, had suffered for about two months from pain, with swelling and fluctuation of several joints, particularly the knees. The right knee was more especially complained of; and there was considerable effusion into the joints, evinced by a soft, fluctuating projection on either side of the patella, and across the lower part of the thigh, just above the knee. He took, each night, Acetic Extract of Colchicum, in doses of three grains; and Acetate of Morphia, which last was increased gradually from a quarter to a half a grain, but without any effect in relieving the affected part. About the end of May, 1837, he complained of his memory being impaired, so that he had great difficulty in remembering words; but he was able to go out in an open carriage, and took a short airing on Sunday, June 2nd. Next day I found him in bed, unable to answer questions otherwise than by the monosyllables, ‘Yes’ or ‘No.’ He expressed his

mortification at this inability to speak by gestures ; and when asked if he had any pain or giddiness about the head, answered 'No,' and shook his head in such a manner as to show that he perfectly understood what was said to him. His pulse was 78, and soft : his tongue clean. No complaint was made of the knee ; but on examining it, I found the swelling almost entirely gone ; indeed all fluctuation had disappeared, and there remained only some puffiness about the parts. He was freely purged, and a mustard poultice applied to the knee.

"4th.—The same symptoms continued without perceptible change. Twelve leeches were applied to the temples, and the purging repeated.

"5th.—In the early part of the day he seemed better, recollecting his sister's name, which he had previously forgotten. Late at night was seized with a fit of screaming, accompanied by strabismus, affecting the right eye, and followed by frequent moaning.

"Leeches to forehead, and gr. ij of Calomel every three hours. A blister to the back of the neck.

"6th.—Strabismus gone, but mouth perceptibly, though slightly drawn to right side, with much subsultus tendinum, and frequent sighing. Answers questions which require only monosyllables distinctly ; says 'No,' when he is asked if he has pains in the head ; and 'Yes,' when asked if he has any giddiness. Pulse 80, with some sharpness. Twelve leeches to temples ; Calomel, gr. iij, every three hours. Strong mercurial ointment to be rubbed in very freely.

"7th.—Very little change. Calomel omitted after seventh dose, in consequence of purging. Mercurial frictions continued every two hours.

"8th.—Much more sensible, and expression improved. Urine, which had been rather scanty, has become more abundant. Pulse 78. No mercurial fetor, but a perceptible red line on the gums.

"9th.—Continues, apparently, to improve; gums decidedly injected, but there is no mercurial fetor. Takes beef-tea plentifully. Shows, by his manner, that he recognises those about him, but cannot name even his most familiar friends.

"Towards evening he began to sink; frequent starting, and constant deep sighing; pulse became diminished in power, while it rose in frequency to 100. These symptoms continued till the evening of the 10th, when he expired.

"*Autopsy.*—The convolutions of the brain were flatter than usual; the arachnoid was injected, and there was a slight appearance of effusion. The ventricles contained from one ounce and a-half to two ounces of clear limpid serum, and each corpus striatum had the appearance of being smeared over with a thin layer, like cream; but this could not be wiped off, and apparently depended on thickening and opacity of the lining membrane. The convolutions of the left *fissura Sylvii* were adherent, from inflammation of the interposed membrane; and the substance of the brain, on either side of the fissures, was of a yellowish colour, with patches like minute points of extravasation. The heart and other thoracic viscera were

perfectly healthy. In the right knee, which had been the chief seat of the rheumatism, the membrane was thickened, rugose, and red all round, close up to the cartilages. The membrane had been pushed upwards, so as to increase the extent of the cavity, but no longer contained any preternatural quantity of synovia. Now this is a case to which I have seen several more or less analogous, and evidently consisted in a slow and treacherous inflammation about the brain, producing fatal effects, without exciting the general system into any of that sympathy which usually attends the progress of mortal diseases.

“In another form of this metastasis the symptoms are more marked, especially as regards the occurrence of pain in the head, by which the seat of attack is clearly pointed out. A case of this kind occurred some time ago at St. George’s Hospital, under the care of Dr. Seymour. A man had long laboured under synovial rheumatism, and there was great permanent distension of the capsules. One day, the knees, which had been greatly swollen, were observed to be flaccid—reduced, in fact, by about one-half. He complained at the same time of pain in the head; became paralysed on one side, and expired in the space of thirty-six hours. On opening the body, a deposit of greenish-looking, purulent matter was found smeared over the greater part of the surface of the left hemisphere, and there was considerable effusion into the ventricles. Here the disease had gone on silently up to a certain point, as there was not the slightest evidence of any increased action about the head till the paralysis came on.

“In yet other cases of this affection, there is pain of the head, of several days’ duration; afterwards giving place to symptoms of pressure, very much in the manner we see in hydrocephalus. A very good illustration of this is contained in Sir B. Brodie’s work on the joints.

“A lad, aged thirteen, had inflammation of one knee, with effusion into the joint; thickening of the synovial membrane resulted, and the enlargement of the joint continued for several months. At the end of this time the swelling became suddenly reduced; and this was simultaneously accompanied by the supervention of acute pain in the head. This returned several times periodically at night, and after a week entirely ceased, when he became affected with somnolency, strabismus, and partial blindness; in which state he died.

“In almost every instance wherein the cerebral affection assumes the insidious form described in the first case, the paralytic attack detailed in the second, or the form of hydrocephalus, as in the third, the attack proves fatal. But, in some, the disease more resembles common inflammation of the brain; admits of the ordinary active treatment; and, more frequently than in the preceding instances, also admits of a successful termination.”

It is true that the cases just quoted were all those of capsular rheumatism, and that Dr. McLeod makes an exception in favour of this form of disease, in reference to its tendency to metastasis. Still we have the fact of a transference of morbid action from the knee-

joints to the brain, established in regard to at least one kind of acute rheumatism; and when we find, in ordinary cases, that the inflammatory action shifts from joint to joint—now attacking one ankle and knee, then similar joints on the opposite side—again suddenly leaving the lower limbs free from pain, and settling upon the wrists and elbows—what inference are we to draw, when, instead of pain and swelling of any joints, we have perturbed action of the brain? Supposing the brain to be affected with rheumatic inflammation, how can it manifest its presence? Not by redness and swelling, as the limbs do, nor necessarily by pain, but by delirium. There is no other reasonable explanation of the occurrence of cerebral symptoms in the case before us; for the supposition of their arising from sympathy between the brain and inflamed heart, is excluded by the fact, that it was not till some days after the delirium that there was any cardiac disorder at all: besides, in this case, the symptoms of this affection were much slighter than in many other patients, who showed no trace of any cerebral disturbance. Still, in saying that this was an instance of metastasis of rheumatism, from the extremities to the brain, we have only given a superficial reply to the question; for it may be asked—Was there anything in the state of that organ to predispose it to such an attack? The class of persons liable to the occurrence of delirium during an attack of rheumatism, are, according to Dr. Fuller, “those who are pale, weakly, and unhealthy; who have been much reduced by blood-letting, or by excessive and long-continued

lactation, or during recovery from serious illness;" or who, again, as Dr. Watson has remarked, "experience a relapse after a long and tedious attack of rheumatism." None of these causes, however, had been in operation in the case of this patient. She was robust, and cannot be said to have been unhealthy; she had not been weakened by depletion, or by profuse sweats, and she had only been a few days affected with rheumatism when the delirium manifested itself.

I conceive that, in Ann Brewer's case, the explanation is to be found in the peculiar condition of her system, which gave rise to the diarrhœa. This is a very rare complication of acute rheumatism; so rare, indeed, as not to be noticed either by Dr. McLeod or Dr. Fuller. I never met with it before. But the diarrhœa, in itself, would not have been sufficient to account for the cerebral predisposition, since the discharge from the bowels was not considerable till after the delirium had appeared. It was not the diarrhœa, but the constitutional derangement, which showed itself in the *brown tongue* and intestinal irritation, as well as in a peculiar dull expression of countenance, much more like that of a patient suffering from the incipient stage of typhus, than of rheumatic fever. In fact, if it were possible to imagine a hybrid between these two diseases, this case might have been so designated. The same causes that induced this intestinal irritation, also produced a condition of profound depression on the nervous system, and gave rise to extreme debility and feebleness of the brain, which disabled it from bearing with impunity the presence of blood

loaded with lactic acid. Such seems the most satisfactory pathological explanation of this very unusual and alarming series of symptoms. That the case was one of the greatest danger is certain. Of such cases, Dr. Fuller writes—"Thus, then, as head symptoms are invariably connected with great susceptibility of the nervous centres, are usually accompanied by acute inflammation of some internal organ, and occur almost always in persons of a weak, excitable habit, they are always indicative of *extreme danger*, even when unattended by cerebral inflammation. *That recovery does take place occasionally*, I am satisfied, both by personal experience and by the testimony of others who have watched and noted cases in point; but when the delirium is violent and of long continuance, the result cannot be otherwise than doubtful. Whether the recovery does ever take place when the delirium is dependent upon cerebral inflammation, it is impossible from experience to describe, inasmuch as convalescence itself forms a bar against our only source of positive information. But if in any case, accompanied by symptoms of acute cerebral disturbance, no cardiac or pulmonary disease should be detected, it would be right, *cæteris paribus*, to speak more decidedly as to an unfavourable issue, than if the stethoscope had revealed mischief within the chest." (p. 296.) In the case under consideration, there was no cardiac or pulmonary affection present when the delirium came on. The delirium may be said to have been violent, for it required the utmost efforts of an attendant to prevent her getting out of bed; at all events, it was not the

mere low muttering delirium of slight disturbance of the cerebral functions. That it was not of long continuance, may fairly be ascribed to the treatment. Indeed, the course and issue of this case cannot but be considered as highly favourable, and as affording strong evidence of the *positive* efficacy of the medicines administered. In her former attack, which was apparently uncomplicated by either cardiac or any other affection, and which was treated *negatively*—that is, by flannel, rest, and mild purgatives—she was confined to the house for two months. Her recent attack may be reckoned to have begun on the 8th or 9th of February; and on the 11th of March she was entirely free of all morbid symptoms, except a slight bruit, which disappeared during the next fortnight. Thus, while recovery from simple rheumatic fever took two months, under Nature, recovery in the same patient in a weaker state from the same disease—not simple, but attended with the most serious complications both of the heart and of the brain—may be said to have taken place in a little more than one month, under Homœopathic treatment.

Let us now proceed to consider the treatment in detail. On her admission, as the symptoms were those of acute rheumatism—namely, severe pain of the joints; a full pulse, of 120 in the minute, and moist skin—the usual medicines, Aconite and Bryony, were prescribed. In forty-eight hours the pain was gone and the pulse reduced, and there was every appearance of a speedy recovery. Within a few hours, however, instead of advancing towards health, the patient sud-

denly became delirious; still Aconite and Bryony were continued. Of the suitability of Aconite for acute rheumatism, complicated with cerebral affection, there can be no doubt, for the action of this medicine upon the brain is very decided; indeed it is our sheet-anchor in almost all cases of threatened inflammatory action of that organ; and its value in such cases is so thoroughly known and universally acknowledged, that I need not adduce any evidence in its favour, either from pathogenetic experiments, or from our clinical records of its curative power.

The action of Bryony on the brain is also satisfactorily attested by numerous trustworthy observers. Professor Zlatarovich, who has published "a proving" of this medicine in the Austrian Journal, in his summary of its action, specially mentions congestion and inflammation of the brain. He also quotes the following interesting case in illustration:—"An officer of excise, whose duty required him to be exposed to the weather all night, having fallen asleep in the snow, remained there for six hours. On being awakened he could not stand from general weakness and stiffness of the whole body. After having been assisted to his quarters, he was attacked with violent rigors, which lasted for six hours; Dr. Schüler was then sent for. He found the patient trembling from cold, his face red, and his head hot, notwithstanding long and most persevering efforts that had been employed to restore natural warmth to the whole body. Although he was oppressed with ~~some~~ somnolence he could not sleep, except for a few ~~minutes~~ minutes at a time, and awoke in a state of violent

delirium, and suffering at the same time from pain in the limbs and head. Dr. Schüler administered a drop of the tincture of Bryony. The first effects of this (too strong) dose was a severe aggravation of all his symptoms. In the course of two hours, however, there was a manifest mitigation of the delirium: he asked for drink, and drank copiously of water. Most profuse diaphoresis then took place, so that five times his night-dress had to be changed, as each one was saturated with perspiration having a strong peculiar odour. On the following morning he was perfectly well, except being very weak; he had no return of rigor, and in a few days was able to resume his occupation.”* I have quoted this case because it seems to me probable, that but for the timely administration of Bryony, the patient would have suffered from an attack of regular acute rheumatism, very possibly accompanied with delirium; all the conditions required for such a combination being present. At all events, nothing can better illustrate the action of Bryony upon the brain when in a state of congestion and incipient inflammation.

The delirium abated under the use of these medicines, and on the following day the most troublesome symptom was the diarrhoea: *Rhus Toxicodendron* was then prescribed. This medicine was taken for three days, and with the best effect; at the end of that period the delirium had entirely gone; but, alternating with the excitement, there was perpetual and profound somnolence; the patient slept constantly; and could hardly

* *Oster. Zeitschrift für Hom.*, band iii., p. 116.

be awakened ; this remained after the nocturnal delirium had entirely subsided. The whole of the symptoms were pretty well covered by *Rhus* ; and it would have been continued, but that, upon the following day (the 18th), the pulse had risen from 104 to 120. The patient complained of pain over the region of the heart, and, for the first time since her admission, a slight systolic murmur was detected, accompanied with slight but unmistakable friction-sound. So we now had both endocarditis and pericarditis, accompanied with great oppression of the breathing. In these circumstances Mercury was substituted for *Rhus*. She took two grains of the second trituration of *Mercurius Solubilis* every two hours for the next two days. On examining the heart at the expiration of this period, the friction-sounds could not be detected—another example of the rapid effect of Mercury in subduing the plastic inflammatory action. The medicine did not seem to have exercised so much influence over the morbid condition of the valves, for the murmur was more distinct. On the 31st the patient was in a most critical condition ; there was much cough and embarrassment of the breathing ; on auscultation, low mucous râles were everywhere audible. The tongue was dry and brown ; the bowels still very relaxed, and the weakness so great that she could not move. The pulse was 120. The medicines now prescribed were Aconite and Pulsatilla. After taking them for two days there was a marked improvement in the general features of the case. The tongue had become moister, the stools less frequent, and the cough had abated.

The indication for *Pulsatilla* was the persistence of the diarrhoea and the catarrh; the wandering character of the pains, as well as the state of the mucous membrane of the bronchial tubes, and the hurried respiration—this oppression of the respiration being functional, and not organic. As we shall have other opportunities for investigating the action of *Pulsatilla* upon various forms of rheumatism, and as the reason for its administration in this instance was the accidental complication of diarrhoea and catarrh, it is unnecessary to say more about it than that the improvement of the patient was so decided the two next days, that no change was made in the treatment for a week. The report then shows that the purging had ceased; that the tongue was moist and natural; that there was no perspiration, and no pain; and that the only remaining symptom was intense prostration. The patient, it should be observed, was of a marked leuco-phlegmatic temperament, bland and stout, and generally quite of a *Pulsatilla* disposition.

Phosphorus was now prescribed on account of the intense prostration. The weakness seemed chiefly to affect the brain and nervous system. There was no great emaciation, nor was the pulse very low, as it is after typhus fever; but the patient was not able to sit up in bed for nine days after all active disease had been subdued. This was on the 9th of March. She was then free from all morbid symptoms, except a slight systolic bruit. For this, *Naja Tripudians* was prescribed, and continued for a fortnight. She was then perfectly well. The pulse was 80; the heart's action

normal; not a trace of bruit remained; her strength rapidly improved, and she was dismissed in as good health as before her attack.

This case made a deep impression on all who watched it. During the six weeks the patient was in the hospital, she underwent all the ordinary, and one very unusual complication of acute rheumatism, without any of them leaving a trace of bad effects behind. To suppose she could have recovered from all these distinct attacks of various forms of disease spontaneously, is too great a demand upon our credulity. Even the most ardent worshippers of Nature, would hardly have ventured to have left such a case as this to her unassisted efforts; and in the absence of Homœopathic medicines, what course of treatment one should have prescribed is a question which frequently arose, and is a natural subject of reflection.

“In its simplest form and early stage,” writes Dr. Barlow, in the *Cyclopædia of Practical Medicine*, “acute rheumatism is speedily and effectually relieved by antiphlogistic treatment, properly directed and adequately pursued. Bleeding, purging, salines, with Antimony, diluents and abstinence, suffice both for subduing the general fever, and removing the local inflammation. In well-marked cases of rheumatic fever, within the first week of their onset, and in individuals of the average degree of robustness, from twelve to twenty ounces of blood may be abstracted with advantage, several successive times, in the course of five or six days.” Such are the practical directions given by Dr. McLeod, who is still one of the standard

authorities on the subject of which he treats. Let us observe, *en passant*, that there is no question, in regard to acute rheumatism, of any *change of type*—the convenient evasion of the old school, when pressed hard upon their abandonment of venesection in acute cases, influenced really, as they are, by the well-established success of Homœopathy in this class of diseases. There is not a particle of evidence to raise, even to the rank of probability, the confident assertion we hear constantly repeated, that all acute diseases have so changed their type, as to be no longer amenable to the same depleting treatment which was successfully directed against them a few years ago. The statement is utterly opposed to all modern pathology, which more and more demonstrates the unchangeable character of the results of diseased action upon the various organs affected by it. The rheumatic fever of Sydenham no way differs from the acute rheumatism of McLeod. And for the two hundred years that intervene between these two practitioners, blood-letting has maintained its place in the fore-front of the battle against this natural enemy of England. There was nothing in the case of this girl, on her admission into the hospital, to suggest any exceptional risk in the employment of venesection. The slight diarrhœa which was present, might be considered as a substitute for the purgative with which it is usual to accompany depletion. It could not have been regarded as a sufficient substitute for the use of the lancet. Suppose, then, she had been bled to the very limited extent of ten ounces upon the 11th, 12th and 13th; in all, she would have lost

thirty ounces of blood when the delirium set in. If this delirium had been owing to intense local inflammation of the joints or heart, it may be conjectured that blood-letting might, by extinguishing the cause, have prevented the effect; but that was not the case; the affection of the brain was a metastasis to that organ, from some source of weakness there; and we know that, so far from doing good in such circumstances, blood-letting would be certain to do evil; and had we bled her, we should have reproached ourselves, with justice, for having brought on the enfeebled condition of the brain, which led to this usually fatal termination; and, moreover, if she had been bled, there can be no doubt her chances of recovery from this most dreadful complication would have been immensely diminished. In one of the few cases of this kind, given in detail by Dr. Fuller, he ascribes the cerebral symptoms to a state of "excitement without power;" and the treatment found successful, and which he recommends in similar complications, is the administration of a stimulant in combination with a sedative—Sulphuric Ether and Morphia. Suppose that, by the nice adjustment of these two antagonistic agents, we had succeeded in calming as well as strengthening the brain, how should we have dealt with the next eruption of the enemy? We must have suspended the stimulants at the risk of the brain, or persevered in their use at the risk of increasing the mischief at the heart, and all the time have been accusing ourselves as partly the authors of the danger to the patient and our own perplexity. Depletion in

this case, we are fully justified in considering as inadmissible, seeing that, even without it, the exhaustion of the strength was so great as for long to be itself a source of the greatest anxiety.

In a former lecture I expressed my approval of the alkaline method of treatment. It at least has the merit of being rational, if only palliative. But no one surely would have ventured to give a large dose of saline draughts, every four or five hours, to a patient suffering from watery diarrhoea to such an extent as to endanger her life? And here let me observe, that although much may be said in favour of the treatment by alkalies, yet it is essentially a superficial method, and has this great drawback—that it leaves us in the lurch in exactly the most difficult circumstances. In plain-sailing cases of simple acute rheumatism, it may answer very well; but when the heart is much affected, and the treatment has to be directed chiefly to arrest the mischief in that organ, as in the instance related in the last lecture, much could not be expected from the administration of alkalies in large quantities; still less good, if not positive injury, should we anticipate, in cases where the brain was affected; and when such a complication as this occurs, of affection of heart and brain, along with diarrhoea, the saline method would break down utterly. It is impossible not to be struck with the penury of allopathic therapeutics, when we attempt to apply them to the treatment of cases of real difficulty; and we can hardly wonder, that when so much can be said against each particular remedial appliance, whether it be venesection

tion, or purgation, or diaphoresis, or the administration of alkalies, or of Calomel and Opium, or of Colchicum, not only on the ground of each of these being of questionable advantage, but on the much more serious allegation that all of them, if not useful, are noxious;—we can hardly be surprised that, in the face of such danger of doing harm, the older and more experienced practitioners should shrink more and more from active measures, and satisfy their conscience, at least in a negative way, by abstaining from doing positive injury to the patients who commit themselves to their care, by recommitting them to the care of Nature. It is not with any desire to extol the new method at the expense of the old that I make these reflections; but as there was for long an imminent peril to the life of this poor girl, it became our duty to consider what other means besides Homœopathic remedies might be available. None other were employed, and the result shows that none were required; but even had the issue been unfavourable, I do not think that an impartial medical tribunal (if such there be, either in *posse* or in *esse*) could have imputed blame to us for not adopting the contradictory, uncertain, and hazardous expedients recommended by the standard writers of the allopathic school.

LECTURE IV.

SYPHILITIC RHEUMATISM ; RHEUMATIC GOUT ; RHEUMATIC OPHTHALMIA.

I PROPOSE, in this lecture, to bring under your notice a case of syphilitic rheumatism, a case of rheumatic gout, and a case of rheumatic ophthalmia. The first is that of Joseph M——, aged 29, a sign-painter, admitted into the hospital on the 27th of September, 1859. He states that three years ago he had a chancre, for which he was treated with Mercury, and he afterwards had a copper-coloured eruption over the greater part of his body. He was then treated for secondary syphilis, probably with Hydriodate of Potash, and apparently cured; although the lower third of both legs are still marked with patches of this eruption. Six weeks ago he was attacked with his present symptoms, which have been steadily increasing up to the present time. These are, a small ulcer surrounded with a dull-red margin on the right tonsil, which causes sharp pain on swallowing. He complains of pain in all his joints, but especially in the hips and ankles. There are painful nodes on the tibia; there is swelling of both ankles; the pains are worse at night; there is copious perspiration in the lower parts of the body; he has no appetite, and for some time past has been rapidly losing flesh, so that he is now much emaciated.

Prescription—Kali Bichrom., 6th dil., gutt. 1, ter die.

Sept. 30th.—Throat is better; he now complains of severe pain in the right clavicle, aggravated by pressure, and of pains in the shin-bones.

Prescription—*Daphne Mezereum*, 2nd dil., gutt. 1, ter die.

Oct. 4th.—Great improvement; pain in clavicle much less; has slept better.

7th.—Complains of much pain in the right ankle-joint, and some return of pain in clavicle.

Prescription—*Rhus Tox.*, 3rd dil., gutt. 1, four times a day.

18th.—General improvement; less pain; more appetite.

19th.—Complains of pain in both ankles; there is a puffy swelling before the external malleolus of both sides.

Prescription—*Hepar Sulph.*, 4th dil., a pilule four times a day.

24th.—All the symptoms improved.

On the 1st of November he was discharged, perfectly recovered from all the symptoms of his present attack. The throat was well; he had no pain; his appetite was good; he slept well. The copper-coloured eruption was the only remaining sign of any disorder of his system.

We are warned, by practical authorities, to be on our guard against taking it for granted that all cases of periosteal rheumatism are of a syphilitic origin. Dr. McLeod says—"Many seem to regard almost all cases of nodes as of syphilitic or mercurial origin; but I have seen numerous instances in which I believe, and some in which I am quite satisfied, that neither of these causes have been in operation. What I have looked upon as rheumatism of the periosteum, has been, after exposure to cold, some part where there is a bone beneath, but thinly covered with integument, becomes painful and tender, with some degree of puffiness. It may occur on the cranium or across the bridge of the nose; on the clavicles or sternum, more especially the latter; and on the extremities, more particularly the tibia and the ulna." After describing the chronic form of the complaint, he adds—"This variety of the disease is chiefly met with in persons of debilitated frame, and having a cachectic state of the system—circumstances which have, no doubt, contributed to the disease being somewhat unhesitatingly attributed to other causes than rheumatism. I have met with this disease, however, in some whose constitutions evinced none of the circumstances alluded to, and where the whole history of the cases left no doubt on my mind of these being rheumatic."

* Dr. Fuller says—"There is yet another form of rheumatism which is apt to occur in cachectic conditions of the system, and is well deserving of especial notice: I mean that known as periosteal rheumatism. * * * This form of disease is frequently described as occur-

ring only among those who are tainted with venereal poison, or whose systems are saturated with Mercury. Nothing, however, can be more erroneous than such a statement. True it is, as already stated, that this form of the complaint is most common in cachectic states of the system, and is therefore met with most frequently—nay, almost exclusively—among those who have been depressed by the operation of the syphilitic poison, or by long-continued mercurial action. But it is not necessarily confined to cases of this sort. I have known inflammation of the periosteum occur during the course of a well-developed attack of acute rheumatism; and, on several occasions, have met with nodes in persons among whom I am convinced no such agencies had been at work, and in some of whom it is morally certain they never could have been. Even the existence of a cachectic state of system does not seem to be a condition *essential* to its development, though the disease is certainly most common among, nay is usually confined to, those in whom a state of cachexia exists.”

I have quoted these two passages because there is no error fraught with direr consequences than attributing a simple affection to a syphilitic origin. A hastily-formed and rashly-announced opinion on this point may be a source of life-long bitterness to the patient so wronged, and of not altogether undeserved disgrace to the practitioner. Besides the moral importance of accuracy and caution in forming our judgment in this class of cases, the question has considerable scientific interest; for we naturally ask whether syphilis has the

power of combining with rheumatism, or merely of predisposing, like any weakening agent, certain tissues to be attacked by rheumatic action? It would occupy too much time to pursue this inquiry here, and to point out its importance in reference to therapeutics.

In the case before us, we can hardly doubt, that whether as a predisposing or a proximate cause, syphilis had to do with the symptoms presented by this patient. In the first place, not many years before, he had, by his own acknowledgment, been treated for secondary syphilis, and he never had lost the peculiar coppery eruption indicative of that disease. On his admission, besides rheumatic symptoms in his limbs, his throat presented the well-marked appearances given by syphilis or Mercury, or the joint action of both.

The first medicine prescribed in this case was the Bichromate of Potash. We have every reason to expect good from this remedy in syphilitic rheumatism. Dr. Drysdale, to whom we are indebted for the introduction of this powerful drug into the *Materia Medica*, thus expresses his opinion in reference to its relation to syphilis:—"The resemblance, in many respects, between the action of this medicine and that of syphilitic virus, and also its analogy to Mercury, would lead us to hope that we may find in it another remedy for that disease. * * * We have in this remedy the rash on the skin; then the sore throat, which has been mistaken for syphilitic; then the periosteal pains; then the rheumatism; and, lastly, the diseases of the skin, &c." In regard to rheumatism,

Dr. Drysdale writes—"Of its utility in this disease, various examples will be found under the individual organs; it seems most suitable to the chronic form, unaccompanied with much swelling." The special relation of Bichromate of Potash to the throat, naturally induced its selection in the case under consideration. Symptom 77, in the proving, records—"The uvula and tonsils become red, and swelled, and painful, and finally ulcerated, and were suspected by a surgeon to be syphilitic." Mr. Moore, of Liverpool, published a case of syphilitic ulcerated sore throat, in a woman thirty years of age, which had lasted six months when she came under his treatment. After taking the Bichromate of Potash for a week, there was manifest improvement, and, at the expiry of a month, perfect recovery. Dr. Watzke, of Vienna, bears testimony to its efficacy in the cure of those sore throats which follow chancre. The patient whose case we are now considering, took this medicine for three days, and apparently with benefit to his throat, of which he did not again complain during the remainder of his treatment in the hospital.

On September 30th, he complained of "severe pain in the right clavicle, aggravated by pressure, and of pains in the shin-bones." These were evidently periostic pains, and the medicine prescribed was *Daphne Mezereum*. This shrub was introduced to the notice of the profession by Dr. Alexander Russell, physician to St. Thomas's Hospital, about the middle of last century. There is a very interesting essay, by Dr. Russell, in the third volume of the *Medical*

Observations and Inquiries (1757), giving an account of a series of seventeen cases in which he administered decoction of Mezereum. At first he employed it in a very complicated recipe, containing some eight or ten other substances, given him by some one who professed to cure, by its means, syphilitic nodes. With the tact of an inductive philosopher, Dr. Russell gradually simplified this mixture until he arrived at Mezereum as its active therapeutic ingredient; and he defines rather exactly the class of cases for which it is suited. "The diseases," he says, "for which I principally recommend the decoction of the Mezereum-root as a cure, is the venereal node, that proceeds from a thickening of the periosteum from other causes. I have seen very good effects from it; and it is frequently of service in the removal of those nocturnal pains with which venereal patients are affected." In the second volume of the *Archiv*, there is a summary of its action by Stapf; and its full proving is given by Hahnemann in the third volume of his work on Chronic Diseases. The following symptoms there occur:—"Stabbing bone-pain in the clavicle," 460. "Pain in the bones of the thigh and leg," 482. "Severe pain, after midnight, in the shin-bones, as if they had been beaten, or as if the periosteum were torn from them; the pain disturbed the sleep, and was attended with rigor of the whole body, and intense, enduring thirst," 484. "Pressive pain in the right shin-bone, often recurring." The resemblance of the above symptoms to those complained of by our patient is sufficiently obvious; and after taking

a drop of the first dilution three times a day for four days, the pain in the clavicle had almost gone, and we heard no more of the pain in the tibia. I may observe, that Dr. A. Russell, who introduced it, gave it in large doses, and says it was only then it did good. At the same time he seems to have looked upon it as a specific, for he does not propose any theory of its curative power.

On the 7th, the patient complained chiefly of an aching pain in the right ankle, besides some return of the pain in the clavicle; and for this *Rhus Toxicodendron* was prescribed, as the rheumatism seemed disposed to settle upon the ligaments of the ankle-joint; and to remove this form of the complaint, especially when it affects the feet, *Rhus* is well suited. The symptoms which correspond to it are thus recorded by Hahnemann.

722. Stabs as from a knife in the left ankle.

723. Drawing pain in the right ankle.

731. Stabs as from a knife in the *tendo Achilles*; worse when the part is touched, and on lying down.

734. Early in the morning, pain in the foot as if dislocated or sprained.

735. Cramp-like contraction of the inner side of the sole of the foot, lessened by stretching it and bending down the sole.

The administration of *Rhus* was continued from the 7th to the 19th, with marked advantage. The pains diminished, both the general aching over various parts, and the more acute local pains of the clavicle and the ankle; his appetite improved, and he slept well;

altogether, he was very much better ; indeed, all that remained was a dull pain, or rather a sense of uneasiness in both feet, with a puffy swelling of the part. For this *Hepar Sulphuris* was prescribed, chiefly on account of its well-authenticated powers of acting as an antidote to mercurial poisoning ; for the weakness and swelling of the feet were looked upon rather as the remains of a general dyscrasia, than as a fresh outburst of the rheumatic action upon the part now affected. The symptoms recorded by Hahnemann, of the effects of *Hepar Sulphuris* upon the feet and ankle, are as follows :—

205. Great weariness of the feet, especially felt when ascending.

206. Sense of stiffness in the joints of the lower part of the foot, with a feeling of numbness and fullness.

207. At night, after a somewhat restless sleep, and turning in bed, he was affected with a pain as from a blow for half-an-hour, so severe as to make him cry out ; it was felt on the external side of the foot on which he had been lying without suffering previously ; change of posture did not relieve it ; but grasping the foot, and stroking it with the hand, removed the pain.

208. Pain in the lower part of the foot, especially about the joint, as if it were suppurating.

209. Tearing pain in the feet at night.

210. Drawing, burning pain in the feet as far as the bones, of an evening in bed.

212. Swelling of the feet about the malleolus, attended with dyspnoea.

The correspondence of these symptoms with those of our patient will be at once perceived; and so far as we could judge, the medicine produced a beneficial effect; at all events he improved steadily, and was soon perfectly restored to such health as he had before the attack of rheumatism for which he was admitted into the hospital.

The next case I wish to bring under your notice is one of gouty or neuralgic rheumatism. H—— S——, aged 49, a farmer by occupation, was admitted into the hospital on the 18th of June, 1861. He states that, ten years ago, he was laid up for three months with an attack of rheumatic fever, and that since that time he has never been free from rheumatism. The severe attacks generally last from six weeks to two months; seldom less. They occur most frequently in spring and autumn. The wrists and joints of the fingers are much thickened, swollen, and painful; and on the fore-finger of the left hand there is a large patch of chalky deposit. The heart is hypertrophied, the impulse being preternaturally loud and distinct, and felt below the ensiform cartilage. There is no murmur, however, or indication of any affection of the valves. His present attack began two weeks ago, and has continued to increase in severity up to the present time. The pain is of a very sharp, shooting character, like toothache; it is not long stationary in any one place, being felt now in one knee, then in the other knee; again in the elbow and wrist, and finger-joints. It is not worse at night. The pulse is 93. There are nightly sour-smelling, foetid perspirations. The appe-

tite is good; the bowels regular; the urine normal. Sleeps ill.

R. Acon., 2nd dil., gtt. 1, ter die.

June 23rd.—Complains much of the pain in left wrist and elbow. Pulse somewhat reduced, but little or no improvement in the pains.

Bryonia, 3rd dil., gtt. 1, ter die.

24th.—Pulse 84; not much perspiration; pain in elbow and wrist very intense, sharp, shooting; worse at night, so that he cannot sleep.

Pulsatilla, 3rd dil., gtt. 1, ter die.

27th.—Feels himself very much better; says he has not been so well for months. Slept well last night.

Continue Pulsatilla.

29th.—Entirely free from all pain.

Continue medicine.

July 1st.—Discharged.

This is a very interesting case in various aspects; it is highly satisfactory in its results; for the patient—a most intelligent man—had no expectation, from his experience of such attacks, of being so soon relieved: six weeks being the shortest period of any similar one; while this had not lasted a month from the time of its commencement; and in nine days after he had been under treatment he was convalescent.

The case is interesting, too, in a pathological point of view. It was a true hybrid of gout and rheumatism, or rather it was a three-quarters rheumatism and one-quarter gout. Dr. Fuller, indeed, denies in words that there is such a hybrid; but he seems to me to fall into self-contradiction in his attempt to prove his negative affirmation. "Rheumatic gout," he says (p. 332, *note*), "is not a mere variety of gout or of rheumatism, nor is it a compound of the two diseases; it is *essentially* distinct from them both, has a special pathology of its own, and requires a distinctive title." Again, at page 344, he says—"I trust it is made sufficiently clear, by the above statement, that the disease should not be regarded as of a hybrid character; or, in other words, made up in part of rheumatism, in part of gout. It is my firm conviction, that just as true rheumatism and true gout do both sometimes manifest themselves, at different periods of life, in the same individual, so rheumatic gout may arise in a person who either has been, or may hereafter become, subject to true rheumatism or true gout; and that it has *no connection* with either of these diseases, beyond that which attaches to it in virtue of its being a constitutional disorder, producing local manifestations in the joints." How are we to reconcile this expression that rheumatic gout has no connection, except one of analogy, with either rheumatism or gout, with the following passage?—"Sometimes, however, the diagnosis between acute rheumatism and acute rheumatic gout is by no means easy. Indeed, I entertain considerable doubt whether an attack which commences as acute rheu-

matism, *may not change its type* under certain conditions of treatment in constitution, and terminate eventually in rheumatic gout. Certain it is, that I have seen several cases, characterised at first by all the most striking features of acute rheumatism—by the thickly-coated tongue, the loaded urine, the bounding pulse, the profuse, acid, sour-smelling perspiration, and, moreover, by the peculiar redness and inflammation shifting rapidly from joint to joint, and affecting the larger joints in preference to the smaller joints—in which, after the first intensity of the attack has been subdued, a different train of symptoms has arisen. The larger joints have gradually obtained immunity from pain; but the small joints of the hands have become painful, swollen, and, in spite of treatment, permanently enlarged; the articular inflammation, though less urgent in its character, has been extremely obstinate in its continuance, and has lost its distinctive migratory character: the skin has been no longer bathed in perspiration; the urine no longer wasted; and the pulse has changed its character, and, from being full and bounding, has become soft and weak, or irritable. Thus I have repeatedly known patients crippled by *unequivocal rheumatic gout*, which commenced, in the first instance, apparently as a sequel of acute rheumatism.”

There is such an absence of exactness in the choice of the words and the construction of the sentences in this passage, that the reader is left in doubt as to whether the author really perceives an *essential* difference between two forms of morbid affection which he

here describes as passing insensibly the one into the other, and being only different types of the same disease. My object, however, is not to criticise Dr. Fuller's style, but to arrive at a distinct notion of the opinion he holds. And we get at this a little further on, in a passage where he says—"Analogy furnishes strong grounds for belief that the articular mischief is not due to the presence of lactic acid, or whatever may be the *materies morbi* in true rheumatism; whilst the rapidity with which structural disorganisation of the joint supervenes, even when the local action is apparently less acute, and when the general symptoms are certainly not indicative of febrile disturbance, is a further proof that it (rheumatic gout) is essentially distinct from true rheumatism." * * * "The able researches of Dr. Gairdener have rendered it extremely probable that true gout is in some way connected with a variation of the relative quantities of urine and uric acid in the system; and hereafter it may be discovered that rheumatic gout is dependent on some other perversion in the relative proportions of the constituents of the blood. And if this be so; if, in short, the disease be due to a disordered condition of the blood, essentially distinct in its character from that which gives rise to pure gout or pure rheumatism, we are bound, in accordance with sound pathology, to separate it from diseases it so much resembles, and with which it is sometimes intimately blended, and to establish, if possible, *by chemical demonstration*, its claim to be considered a special disorder." So, according to this exponent of the modern chemical pathology,

we have two diseases, so like one another, that even he, with all his experience, sometimes fails to distinguish them; which blend together, and pass insensibly the one into the other; and yet are *essentially* distinct; arise from opposite conditions of the blood; and, mark! require entirely different treatment. What a dilemma our chemical practitioner would have found himself in if called on to treat the case of the patient under consideration. Was this rheumatism, or gout, or neither, but something essentially different? If we accept Dr. Fuller's definition of rheumatic gout:—in favour of rheumatism, we had the high pulse, sour sweats, and *increased action of heart*—a most important feature in forming a differential diagnosis: in favour of gout, we had the neuralgic character of the pain, and the chalky deposits on the hands: in favour of its being rheumatic gout, we had the combination of the two sets of symptoms. Suppose, now, that before we could prescribe, we had been obliged to pronounce the case to be decidedly one of these three *essentially distinct diseases*, and neither of the others; if we had any regard for logic, we must have felt ourselves shut up to treat it on an opposite plan from that which we should have adopted had we called it either of the other two. And what a hard position this would have been for a conscientious man! For, observe, according to Dr. Fuller's own showing, acute rheumatism does pass into rheumatic gout; so that in the same case, at one stage we must treat it on opposite principles from those that should guide us in its treatment at another stage; the two stages being all the

time undistinguishable—at all events very difficult to distinguish.

I have dwelt at considerable length upon these chemical doctrines, because they are very popular, very attractive, and, in my opinion, very dangerous. I believe that the greatest service chemical pathology will render to therapeutics, is affording additional, unchangeable, and therefore trustworthy signs or symptoms, so that we may more accurately adjust, to any case under treatment, the remedy which bears to it the deepest and *most essential* resemblance. But if we allow ourselves to be led by chemical theories into the pursuit of chemical antidotes, we shall fall into the same delusions as those which misled Sylvius de la Boe, and the chemical school of the 17th century.

Thanks to Hahnemann, we are saved from this dilemma. Whether we call the case rheumatism, or gout, or rheumatic gout; whether it depend upon an excess of lactic or of some other acid in the blood, or of no acid at all; in short, whatever its ultimate pathology be, if we follow his directions, and find a medicine which produces a similar totality of symptoms, we may confidently reckon upon its usefulness. The first remedy prescribed was Aconite, as the pulse was high—93—and the symptoms those of acute rheumatism, to which Aconite so accurately corresponds, and which, in its simple form, it almost invariably relieves. The patient took this medicine for four days. Its action on the pulse was decided, but it made little or no impression on the pains. He then got Bryonia for a day, with negative results.

The report of the 24th was—Pulse 84; pain in the elbow and wrist *very* intense, sharp, shooting—worse at night; little perspiration. Pulsatilla was now prescribed, and in five days he was entirely free from pain, and, in fact, convalescent. The action of Pulsatilla in this case was most striking, and made a deep impression on those who watched the treatment. Hahnemann observes, in a note—"The pains produced by Pulsatilla, which are called *tearing*, consist for the most part in transient, drawing tension, which in every case resolves itself into a twitch of the nature of the tension, as if a nerve were pulled out and stretched, and then allowed to relax with a painful jerk." Like all Hahnemann's observations in regard to what is characteristic about any particular medicine, this remark is full of significance, and points to the special relation of Pulsatilla to the nerves of sensation—a relation which makes it specially suitable in neuralgic or gouty rheumatism.

The following symptoms have a close resemblance to the gouty rheumatic pains in the arm of our patient:—
760. A tensive pain in the tendons of the elbow-joint on moving the arm. 761. Repeated attacks of drawing, tearing pain in the bones of the lower arm in the daytime and at night. The pain is as if a nerve were stretched and relaxed with a sudden jerk. 766. Drawing, tearing pain at night in the arms, especially the fingers. 767. In the inner part of the arm, drawing tensive pain as far as the wrist-joint.

All these symptoms exhibit a close analogy between the effects of Pulsatilla and of the cause, whatever

that may be, which produces rheumatic gout. In the case before us, Aconite was employed at the commencement of the attack; and I doubt if Pulsatilla would have acted so well, but for the previous action of Aconite. Without wishing to place too great weight upon what, after all, may only be an imperfect observation, yet I think it is worth pointing out, that while "sour-smelling sweat" is mentioned as one of the effects of Aconite, although copious night-sweats, having an offensive smell, are recorded under Pulsatilla by Hahnemann, yet neither is there sour-sweat mentioned, nor any pain in the region of the heart recorded, as effects of this medicine. So that we should not expect Pulsatilla to be of much service in acute rheumatism. The contrast between the action of Aconite and Bryonia on the one hand, and Pulsatilla on the other, is quite as great as that between acute rheumatism and rheumatic gout; and the day may come when we shall be able to discover what may be called the essential or radical pathology of these morbid phenomena, and find in what relation their cause or causes stand to the action of their remedies: how far the pains produced by Pulsatilla are due to a disturbance in that part of the nervous system which presides over nutrition (and thus a secondary result of its action), and how far it acts directly on the nerves of sensation. But to follow up this inquiry would lead us into the region of hypothesis, and out of the purely practical ground to which our present design confines us. So we shall proceed to the consideration of the next division of our subject.

Rheumatic Ophthalmia.—This term is objected to by no less an authority than Mr. Mackenzie.* “Were it asked,” he says, “what is meant by *rheumatic ophthalmia*, I should reply, that—

“1. I mean, simply, inflammation of the fibrous membrane of the eye (the sclerotica), and of the surrounding parts, of similar structure, excited by exposure to cold.

“2. I do not believe this ophthalmia to be an inflammation differing *in kind* from common inflammation, in consequence of the existence of what has been called the rheumatic habit, or diathesis. When atmospheric influence produces catarrh, we never hear the occurrence referred to a mucous diathesis; nor, when pleuritis arises from the same cause, do we attribute the disease to a serous diathesis. The same exciting cause, affecting a fibrous instead of a mucous or a serous membrane, produces a new train of symptoms, dependent, not on the constitution of the person, but on the structure and functions of the part affected.”

To meet this objection fully, we should require to discuss the distinction between the exciting and proximate causes of the affection under consideration—a distinction which the eminent oculist seems to neglect; for the same exciting cause—viz., atmospheric influence—may give rise to wholly different actions in the same tissue—the difference arising from the condition of the structure at the time. However, instead of entering upon a discussion which would be some-

* *On Diseases of the Eye*, p. 477.

what out of place at the close of a clinical lecture, I prefer, by other quotations from the same author, a few pages further on, to show that, although he objected to the term theoretically, yet he adopts it for practical convenience. "It is reasonable to conclude," he observes, "that, in this disease, the periosteum in and round the orbit, and the fascia of the temporal muscle—structures similar in nature to the sclerotica—are also [observe the *also*] affected with *rheumatism*." Again, he says—"Probably, the same exciting causes which, in persons of middle life and robust constitution, are apt to induce *rheumatic ophthalmia*, would, in a child, excite catarrhal or scrofulous ophthalmia; and, in an old person, the catarrho-rheumatic or arthritic." We see, by these citations, that while Mr. Mackenzie objects to the term, he cannot get on without it; and as it is one commonly received, and universally understood in the same sense, we are quite justified in continuing to employ it to designate a very well-marked class of cases, of which the following is an example:—

T. C., æt. 39, a married man, by occupation a salesman, was admitted into the hospital on the 28th of May, 1862. States that he had an attack of a similar character to what he now suffers from, three years ago, and was treated for it, in the Eye Infirmary, Charing-cross. This was his first attack. He recovered from this attack; and, after some time, had a second. The present is his third attack. It commenced three weeks ago, after exposure to cold. It began with severe aching pain in the eyeball, and

redness of the eye, followed by severe pain round the orbit. He has frequently suffered from rheumatism of the joints. On admission, both eyes are inflamed, the conjunctiva deeply injected, and the iris slightly discoloured; the right eye is the worst, but the left is similarly affected, although not in the same degree. There is lachrymation, and much aching pain—worse at night. He suffers from headache. Pulse 68, rather weak. Tongue clean; bowels regular; appetite good. Urine very scanty, and high-coloured.

Prescription—Merc. Corros., 2nd dil., gtt. ii.; 4th
9. 9. horis.

Merc. Corros., 1st gtt. x.

Aquæ distill. ʒi.

To be used as a lotion.

June 7th.—Inflammation has entirely ceased, but complains of imperfect power of sight.

Bellad., 3, gtt. i. ter die.

9th.—More power in the left eye. Vision of the right very imperfect.

Continue.

11th.—Return of pain, lachrymation, and intolerance of light.

Sulph., 30, gtt. ii. bis die.

13th.—Great improvement in both eyes.

Continue.

rotated outwards; and, under these circumstances, we have known partial laxations of the patella occur. The patient usually becomes bent in his figure, the spinal column being flexed forward. The neck often becomes rigid; and this state of things, too, is sometimes found associated with a rigid condition of all the joints—great and small—of the upper extremities. Now, the patient, although he has laid before him food convenient for him, soon becomes really incapable of feeding himself, and thus, in a certain sense, becomes wholly dependent on others.” This is not at all an overdrawn picture of the consequences of chronic rheumatism. Indeed, when we look over the delineations which Dr. Adams gives in the work from which this passage is quoted,* of the degradation of the human form by this disease; of the elongated jaw-bones; the almost horizontal, instead of perpendicular, spinal column; and the incurvated fingers and toes—one could readily imagine a counter theory to that of the development of species arising, viz.—that a process of degeneration was engendered by disease, of such a nature that the human type became gradually metamorphosed into that of some of the inferior animals. However this may be, certain it is that one could hardly find a more melancholy subject of study than that of Dr. Adams; alas! all the more so that, while the writer dwells with the most minute fidelity on all the dreadful contortions which chronic

* *A Treatise on Rheumatic Gout, or Chronic Rheumatic Arthritis, of all the Joints*: by Robert Adams, M.D.; London, 1857.

rheumatism produces, he tells us next to nothing how the disease is to be eradicated, or its frightful consequences averted.

The term *chronic*, used antithetically to acute rheumatism, is open to this objection—that an acute case may linger on for weeks, and be acute all the time; and a chronic case, although it only last days, may yet never be acute. The difference between acute and chronic rheumatism is far deeper than the period of the duration of an attack. The grand distinction between these two forms of disease consists in this, that acute rheumatism is always attended with a febrile condition of the system, while in chronic rheumatism there is comparatively rarely any disturbance of the circulation. Now, much more is implied by the term “febrile condition” than a quickened pulse.

Andral and Gavarret made forty-three analyses of the blood of fourteen patients suffering from acute rheumatism. The result was, that while the mean quantity of fibrine rose to 6·7, healthy blood yielding only three, the blood corpuscles fell to 101·0 instead of being 127, as in health. They also analysed the blood of ten individuals suffering from chronic and sub-acute rheumatism. No peculiarly striking results were obtained. The proportion of fibrine in no instance exceeded 5, and in two cases was as low as 2·9 and 2·6. The blood corpuscles in one instance amounted to no less than 154·3, and the solid constituents to 259·1. In the other cases the corpuscles

were below the healthy average. "These results," observes Simon,* "lead us to the conclusion that, provided there are no other disturbing influences, as the rheumatism loses its acute character, the blood gradually throws off the specific characteristics of hyperinosis."

"In chronic rheumatism," says the same high authority,† "if the pains are not very acute, and the night's rest is not disturbed, the urine retains its normal properties. Out of thirty-seven cases observed by Becquerel, the urine remained unaffected in twenty; while in seventeen it assumed the inflammatory type, and in nine of these threw down a spontaneous sediment."

It is maintained that lactic acid is the *materies morbi* of acute rheumatism; because, in that disease we have all the phenomena produced by a considerable quantity of this poison in the blood; we have a quick pulse, a strong and loud impulse of the heart; we have copious acid perspiration; frequently very acid urine; besides a great excess of fibrine and corresponding deficiency of blood-globules—a ready source of lactic acid. But in chronic rheumatism we have none of these phenomena; the pulse is frequently little, if at all, altered; the heart, unless the attack had begun in the acute form—seldom affected; there is often only a natural moisture in the skin, without any acid reaction: the urine, as we see, is natural, and the blood very little different from the

* Simon's *Chemistry*, vol. i., p. 276.

† Vol. ii., p. 277.

natural standard. Such being the state of things, by what right is it assumed that lactic or any other acid is the *materies morbi* of chronic rheumatism? And, again, what right have we to expect any substantial benefit here from the eliminative treatment? There is no evidence, on the one hand, that there is anything to eliminate; and, on the other hand, when a cure is effected in chronic rheumatism, there is no appearance of there having been any unusual discharge, such as occurs in acute rheumatism.

Let us take a case in illustration:—

Julia C——, age 34, a bookfolder, single, was admitted into Hospital on the 9th of April, 1861. She states that, twenty-six years ago she had an affection of the left hip-joint, which confined her to her bed for a year. She recovered so far as to lose all pain, but there is very little motion in the joint; the thigh is drawn up, and the leg is about five inches shorter than the other. For the last three months she has suffered from pain in the right hip-joint. The pain is very severe at night, so that frequently she cannot remain in bed; it extends down the leg as far as the foot. She has had no sleep, owing to this cause, for the last five nights.

In other respects there is no disorder of the health, except from hæmorrhoids occasionally, and dysmenorrhœa. The pulse and appetite are both normal.

Prescription—*Rhus Tox.*, 3rd, 1, three times a day.

11th.—Has slept better than she has done for weeks.

14th.—Has had rather more pain last night, owing, probably, to the catamenia, which made their appearance to-day.

Continue medicine.

17th.—Improved. Pain much less, and has slept well.

Continue medicine.

25th.—Free of all pain. Convalescent.

Stop medicine.

30th.—Discharged. Cured.

Independently of its speedy cure, there are several points of considerable interest in the case of this patient. There is the old and extinguished disease of the left hip-joint. In this instance, there is hardly room for doubt as to its nature; the patient was only eight years old when she suffered from it, which is of itself proof sufficient that the complaint was a scrofulous, and not a rheumatic, inflammation of the hip-joint. Had it come on at a later period there might be good reason to hesitate as to its character. As it is, we have an interesting example of a person, when young, being affected with a scrofulous disease of the hip, and the same person, when older, being attacked with rheumatic inflammation of the corresponding joint on the other side. If it had not been checked it would probably have become a case of *chronic rheumatic arthritis* of the hip.

This disease, from most frequently occurring in old

people, used to be known by the name of *Morbus Coxæ Sinilis*; but Dr. Adams, having met with instances of it occurring in persons of the age of thirty or forty, has proposed to substitute the former for the latter assignation. The only objection to the name is the word *chronic*; for, from its inception, the disease seems to be of exactly the same nature as it bears throughout; and while, on the one hand, it would be a misnomer to denominate as chronic a complaint of a few weeks' duration, it would, on the other, be a grave error in pathology to designate the same disease by two distinct appellations, just because it was older. The essence of the disease is a sub-acute inflammation of the tissues which compose the hip-joint (giving rise to pain, but not to general fever), and ending in their partial destruction, and a permanent stiffness of the joint, and an apparent shortening of the limb. After the disease has lasted for some time, it presents, according to Dr. Adams, the following symptoms:—

“The patient complains of stiffness in the hip-joint, and about the great trochanter; of a dull, boring pain, which extends down the front of the thigh to the knee. The stiffness is most felt in the morning when the patient commences to walk; but, after some exercise, the movements of the joint become more free. Should the patient have walked very much during the day, the pain is always more severe in the evening; the uneasiness, however, gradually subsides after he has retired to bed. When the patient throws the weight of his body fully on the affected joint the pain is always increased; but if

the surgeon press upon the great trochanter, or adopt any other expedient, so as to push the head of the bone even rudely against the acetabulum, these manœuvres are the sources of no uneasiness whatever to the patient. Although we can easily satisfy ourselves that no actual ankylosis exists, still it is evident enough that the motion of rotation is lost, and that flexion is confined within very narrow limits. When we place the patient in a horizontal position, and endeavour to communicate any of the movements to the hip-joint, he complains of some pain, and an evident crepitation can be heard and felt deep in the articulation. The affected limb has the appearance of being two or three inches shorter than the other; while, on accurate measurement, the real shortening will be found not to amount to an inch. This greater appearance of shortening than exists in reality arises from the obliquity of position of the pelvis relatively to the spine; and the elevation of the former at the affected side is such, that, in the ordinary attitude of standing, the crest of the ileum and the last short rib approach nearer to each other at this side by two inches than do those of the opposite side. The patient walks very lame, and with the foot and whole limb greatly everted. The nates of the sound side is unusually prominent, while that of the affected one is quite flat, and no trace of the lower fold of the gluteus is seen. The muscles of the thigh also seem somewhat atrophied; still they do not want for firmness; and we may uniformly observe that the calf of the affected limb is not inferior in size and

firmness to the other. When we minutely examine the great trochanter, we find it larger and more prominent than usual; and about the situation of the acetabulum, the horizontal branch of the os pubis, and the lesser trochanter's long protuberances can, upon careful examination, usually be recognised. The disease generally appears as a local complaint; and, when once firmly established in the hip-joint, rarely extends to other joints. The inflammation of the various structures of the hip-joint, in which the disease consists, is seldom accompanied by any appreciable degree of heat, or external swelling of the soft parts."

It seems almost to border on the ludicrous to bring into juxta-position the apparently trifling case of our patient and this description of the effects of long-continued and fully-developed mischief in the hip-joint, given by Dr. Adams; but, if we consider the matter, we shall come to the conclusion that these chronic cases must have passed through a stage similar to that of this girl when she presented herself for treatment. For five months she had suffered from pain in the hip-joint of a rheumatic character. Was there, then, any reason why this pain should not continue for other five months, or five years, and so the disease become truly chronic and formidable? and is the fact of her being cured in three weeks by a specific medicine, any evidence of the *essentially* slight nature of the affection? Certainly not! And this—which is one of our strongest points—the rapidity of the cure of the most serious diseases, when

they are subjected to treatment at a sufficiently early stage, is most unfairly used by those who seek to detract from the merits of the system we follow, as an argument against us. "How," they exclaim, "can we believe that a really dangerous disease is thus suddenly cured?" We might reply, "You might as well maintain that a bombshell, if discharged, could never have done any mischief; although the only reason why it did not explode was, that the fuse was extinguished before it communicated with the powder within."

The next case which I wish to submit to your consideration is a very fair specimen of the common form of chronic articular rheumatism.

F. B., Oct. 26.—A female, single, was admitted into the hospital on the 27th of September, 1861.

The patient states that her present complaint began a year ago, with severe pain, and much swelling of the second metacarpo-phalangeal joint of the right hand. Thence it gradually extended to all the other joints of that hand, and into several joints of the left hand. Six months ago her knees became affected in a similar manner. Since then she has been almost entirely confined to bed, and has been in constant pain, more or less severe. Her powers of locomotion are very limited, and she cannot move a step except with the help of crutches. She is much emaciated. The pulse is 120, small, soft, rather sharp.

The tongue is thickly furred and moist. The appetite is moderate. There is no thirst. The bowels are

regular. The urine becomes thick on standing, with pink sediment. The catamenia have not returned since her last confinement, a year ago. She nursed her child for two months, when the secretion of milk ceased.

Prescription—Pulsatilla, 3, gtt. ii. four times a day.

Oct. 2nd.—Copious perspiration has come on.

Sulphur, 3rd dil., three times a day.

7th.—Diarrhoea, with griping pain in bowels; the evacuations are watery.

Veratrum, 3rd, a drop every three hours.

9th.—Bowels still relaxed; much sharp, griping pain.

Colocynth, 3rd dil., a drop every three hours.

11th.—Quite free from pain in the bowels; no diarrhoea. Rheumatic pains rather less.

[I may mention that there was a sudden invasion of diarrhoea in the hospital at this time; almost all the nurses and the porters, as well as the patients, were affected by it. It was generally subdued by Veratrum and Colocynth.]

The treatment for the rheumatism was now resumed, and the patient was ordered

Sulphur, 3, a drop three times a day.

Oct. 15th.—Improved; less pain in joints.

Continue.

21st.—More pain in right hand and left knee.

Ledum Palustre, 3, a drop three times a day.

25th.—Much less pain in all the joints. Is now able to sleep more. Pulse 100, full.

Continue medicine.

28th.—There is much less swelling and pain of the left wrist, and more power of motion in the fingers of the left hand. She can close the hand, which she has not been able to do for months. Pulse 96, full. There is less general pain, and she feels much better.

Continue medicine.

Nov. 8th.—She is now entirely free from all pain. She can flex the fingers and bend the wrist backward and forward.

Sacch. Lactis.

9th.—Complains of pain in her foot.

Continue.

11th.—Pain nearly gone from foot.

13th.—More pain and swelling of right thumb.

Bryonia, 3, a drop three times a day.

18th.—Pain rather increased generally.

Sulph., 3, a drop three times a day.

Nov. 20th.—Gnawing pain in all the joints ; worse on moving them. Rather more perspiration the last two days. Pain in lower part of abdomen and back.

Pulsatilla, 3, two drops daily.

25th.—Less pain in joints. Great general improvement.

Continue.

Dec. 2nd.—More pain in joints. Frontal headache. Little appetite. Bowels confined.

Nux Vomica, 4, a drop three times a day.

12th.—General health better. Still complains of pain in the joints of the hands.

Pulsatilla, 3.

27th.—Discharged much improved. Can move all her joints more, and with less pain, and is generally comparatively free of pain.

I have quoted this case chiefly to illustrate the insidious way in which chronic arthritis attacks its victim, and to direct your attention to the rapid and unequivocal benefit derived from at least two of our great remedies in this class of disorders. I mean Sulphur and *Ledum Palustre*.

Although the pulse was high, I consider that this was, *from the first*, a case of chronic rheumatism, affecting the various tissues that enter into the composition of the joints of the wrists, hands, and knees.

After taking Sulphur for some days, there was a marked improvement. I have observed this almost constantly to be the case in this form of rheumatism. I remember, many years ago, asking a poor woman, long crippled by rheumatism, whether anything had ever done her good. She replied, in her native Scotch dialect—"Weel, sir, the only thing that ever did me gude was what a travellin' packman bad me tak—that was, a teaspoonful of brimstone, in a wine-glassful of whiskey, every mornin', as sune as I was awake." "And what did it do to you?" I asked. "Weel, sir, the first time it made me *uncommon fou*"—*i.e.*, drunk. However, it does not take long to inure a Scotch stomach to a morning dram, and very soon it ceased to produce its intoxication, and she found herself much better of all her pains. On this point, Dr. Fuller writes—"Sulphur is an agent which sometimes proves peculiarly valuable in the treatment of chronic rheumatism, especially when the skin is inactive. Amongst the poor it has long been a favourite remedy, and, by its action, has doubtless contributed not a little to the reputation of the powder termed Chelsea Pensioner, of which it forms a large proportion.

"The following is the recipe of Chelsea Pensioner :—

"Take of Flowers of Sulphur, ℥ii.

Cream of Tartar, ℥i.

. Powdered Rhubarb, ℥ii.

Guaiacum, ℥i.

Clarified Honey, lb. j.

"One Nutmeg, finely powdered. Mix the ingredients.

"Two large teaspoonfuls to be taken night and morning, and to be persevered in till the whole is consumed. For the first three nights, a large tumbler of rum and water to be taken at bedtime; or, if the fever is present, white wine instead of rum." Not very unlike the whiskey and brimstone cure.

To continue the quotation from Dr Fuller:—

"Administered in half-drachm doses three or four times a day, and aided in its action by the stimulating effect of simple vapour, or the sulphur vapour-bath, it is often extremely efficacious in removing chronic muscular pains; and there are few remedies within the range of the pharmacopœia in which so much reliance can be placed. Its use, however, should be restricted to muscular and neuralgic rheumatism; for, in the articular form of the disease, accompanied by synovial effusion, and in periosteal rheumatism, it is far inferior to other remedies; if, indeed, it be not altogether valueless" (p. 418).

I entirely disagree with Dr. Fuller in this restriction of the benefit of Sulphur to cases of muscular rheumatism. It is opposed to my experience, for I have repeatedly seen the most decided advantage derived from this medicine by patients suffering from the true chronic articular form of the malady. In corroboration of my opinion, and in opposition to the statement of Dr. Fuller, I may refer to a very striking case of chronic rheumatic arthritis of the ankle and foot, described and delineated by Dr. Adams in the work already quoted. The person was a patient in

the Richmond Hospital. The description given is as follows:—"On viewing his ankles from before, their unusual breadth across between the malleoli first attracted our notice. The instep seemed depressed, the foot flattened, and the inside of the head of the astragalus and navicular bones were observed to be unduly projecting inwards. The measurement across the tarsus in this situation was found to be, from the right foot, $10\frac{3}{4}$ inches, and from the left foot one quarter of an inch less." It is plain that so much distortion could not have taken place except as the result of inflammation of the synovial membrane and effusion into the joint. The condition for which Dr. Fuller rejects Sulphur. Now let us hear what this patient himself reported of the various remedies he had taken. "Upon referring to his own opinion upon the effects of different medicines which had been given to him, he replied, that he considered the Hydriodate of Potash mixture, although he had persevered in its use for two months, did not afford him any benefit; that his own impression was, he derived more decided good from the continued use of the Chelsea Pensioner's Electuary than from any medicine he had taken during his sojourn in the hospital" (p. 258). Considering the very little space or attention bestowed by Dr. Adams upon the treatment of the cases he describes, the fact of his recording the reply of this patient, is evidence that it made a strong impression on the mind of this distinguished surgeon, who has had great experience in this class of cases, and who, doubtless, had he shared Dr. Fuller's incredulity, would have had no hesitation in

expressing his disbelief that the Chelsea Pensioner had done the man any good.

I do not think it necessary to go over the proving of Sulphur, to show how its pathogenesis corresponds with the symptoms of chronic rheumatism. This, from the number of organs the survey must embrace, would be a tedious task, and one better performed at your own leisure. Suffice it to say, that I almost always commence the treatment of chronic rheumatism by the administration of Sulphur in some form, and that sometimes I find it useful to persevere with this one remedy for months. I believe that in this I merely act in accordance with the general practice of all experienced Homœopathists.

Besides Sulphur, the medicine that did most good in the case under consideration, was *Ledum Palustre*. The patient began it on the 21st of October. On that day she complained much of the pain in her fingers and knees; she continued to take *Ledum* till the 8th of November, a period of eighteen days, when the report was—"She is now entirely free from all pain, and can flex the fingers, and bend the wrist backwards and forwards." She had not been so well for more than six months, and we are fully entitled to attribute the improvement to the use of *Ledum*.

Hahnemann observes, of this medicine, "that it is most suitable for chronic complaints, especially if coldness and a want of animal heat are predominant symptoms." There is nothing more characteristic of chronic rheumatism than coldness. We always find the patient either in bed or sitting close to the fire, partly,

doubtless, owing to the want of power of locomotion ; but in some degree, also, attributable to the imperfect generation of animal heat, for paralytic do not crave artificial heat so much as rheumatic patients. Of the individual symptoms recorded by Hahnemann, of *Ledum*, there are some curiously suggestive of the first stage of chronic arthritis, as, 91. "Drawing pain in the extensor tendons of the left hand." 94, 92. "Stitches in the hand ; some severe, some slight." These sharp, darting pains, are highly characteristic of rheumatic gout. 94. "Tearing pains in the hands." This is also a constant complaint of those suffering from this affection.

Then, in the lower extremities we have, among other symptoms :—101. "Pain in the knees as if beaten, or as if raw. 106. (Pain in both patellas, as if they had been beaten—this pain is felt in walking). 104. Tearing, pressive pain in the right knee-joint, going downwards—worse on moving. 105. Fine tearing pain in the toes of the left foot, especially on their lower surface. 117. Great weariness of the feet, as if he had walked many miles ; only felt while sitting or lying, not when walking." This is a very common symptom in certain forms of chronic rheumatism, in which exercise gives relief for the time ; but after it is over, there is an increase of the pain, and stiffness of the affected parts. Horses suffering from this complaint show lameness only in the stable, or on first coming out of it. I am not aware whether *Ledum* is much used in veterinary practice ; it certainly deserves the attention of veterinary surgeons.

The case I have just narrated was strictly one of arthritis: the complaint began in the joints of the fingers; thence it extended to those of the wrist, and gradually involved the knee; and doubtless, had it not been checked, would have taken possession of the feet and ankles. The following case is an example of *muscular* rheumatism.

Robert Smith, aged 41, a labourer, single, was admitted on the 3rd of September, 1861. The patient states, that about a year ago, when working at a lime-kiln, he got very wet and caught cold; this resulted in a quinsy, which confined him for twenty-seven weeks. After he had recovered from this attack, he began to feel rheumatic pains in his right shoulder, whence it soon extended all down the right side of his body, so that he experienced great pain on moving either the right arm or the right leg. The left side has never been affected. Says that he had syphilis twenty years ago. There is a large node on the right shin-bone, which has been there for many years. On admission he complains of severe pains in the neck, so that he can scarcely move the head. Complains most of the pain about the shoulders, especially the right shoulder, extending down to the elbow and wrist. He also complains much of his right hip. The pain is more in the muscles about the joint than in the joint itself. The pain extends to the knee, which is slightly affected. Pulse 72, full. Bowels regular. Urine thick and high-coloured. Tongue white and coated.

Prescription—*Bryonia Alba*, 3rd dil., a drop three times a day.

5th.—Pain is much better. Is very gloomy, and complains of feeling very unwell.

Aurum Met., 6th dil., a drop three times a day.

12th.—Pain less, but still very bad at night.

Rhus Toxicodendron, 3rd dil., a drop three times a day.

16th.—Great pain in right shoulder.

Sulphur, 3rd dil., a drop three times a day.

23rd.—Still much pain in shoulder; suffers from it all night.

Rhus Tox., 2nd, a drop every four hours.

26th.—He had very little pain in shoulder last night. He has not passed so good a night for nine weeks.

Continue medicine.

Oct. 2nd.—Much better. Very little pain anywhere.

Continue medicine.

9th.—On the whole, much improved. There is redness and ulceration of the gums.

Acid Nitric., 6th dil., one drop three times a day.

11th. The gums are better to-day. Complains of a great deal of pain in the lumbar muscles. Worse when he lies on his side.

Berberis, 2nd dil., two drops three times a day. .

15th.—Pain in back much better. Complains of much pain in the node on the tibia.

Silicia, 12th dil., two drops three times a day.

17th.—Convalescent.

The progress of this case was, on the whole, satisfactory. The medicine from which most apparent benefit was derived was Rhus. At the same time it is not improbable that the Aurum which he took for a week—although its action was not so immediately beneficial—was of service. The indications for it were partly the general state of his constitution. There was still a syphilitic taint in it, which probably gave rise to a marked moroseness of disposition. He was always grumbling and discontented. Besides, some of the symptoms of Aurum bear a close resemblance to some forms of rheumatism, *e. g.* :—

123. “At daybreak, in bed, pain as if beaten in all the joints, especially in the sacral region and in the knee; the pain increases the longer he lies still, let him lie as he will, on the back or side; it goes off, however, soon upon rising.”

130. “Early in the morning she feels very weary. Her limbs pain her, so that she longs to lie down.”

I will not quote further, but merely observe, that if the rheumatism in this case had any connection with syphilis or with its treatment by Mercury, it is interesting to observe that, as he improved in his general health, there was some manifestation of the latent poison being excited to activity, in the state of the gums and of the node on the tibia. This was

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probably due to the action of the Aurum. The pain which he complained of in his loins was rapidly relieved by *Berberis Vulgaris*. The corresponding symptom in the proving of this medicine is thus reported:—"Feeling of lameness, and as if bruised in the small of the back, frequently felt immediately on waking; generally worse when sitting or lying down than when walking; the pain is very frequent and enduring." The medicine is not one which I often prescribe, but I believe it to be quite trustworthy, from what I have observed of its action in conditions for which it was indicated.

The case last related, although chiefly muscular, was partly also articular; and I will conclude this lecture by quoting a very brief case of purely muscular rheumatism.

Thomas Brewer, aged 19, single, was admitted into the hospital on the 13th of December, 1861. He states that six weeks ago he fell out of bed, having previously had a fit of shivering; he was supposed to have had an epileptic attack. From that time he began to suffer severe pain in the back, much increased by motion; also pain down the thigh, chiefly on the outside: these symptoms lasted for three weeks. On the evening of the 11th he caught cold, had an attack of shivering, and was unable to rise on the following morning, owing to the acute pain in the lower part of the back.

On admission he is quite unable to walk alone; any attempt to do so brings on severe shooting pain in the lower dorsal and lumbar region. The pain is situated

rather in the muscles of the back and loins, and is not confined to the spine. The extreme pain is only felt when he attempts to move. When at perfect rest he feels tolerably comfortable, but the parts are tender to the touch. The skin is moist; the tongue clean; appetite good; bowels confined.

Nux. Vom., 3rd, one drop three times a day.

• 17th. He is greatly improved, and is able to walk without assistance.

Bryon. Alba, 3, a drop three times a day.

19th. He is quite well of his lumbago.

The *cure* in this instance—for we may safely assume that lumbago would not have thus rapidly disappeared spontaneously—was due chiefly to Nux Vomica, a medicine more suitable for the treatment of muscular than arthritic rheumatism. Symptom 784 has a near resemblance to the state of this patient; viz.—“Pain, as if beaten, in the muscles of the back and abdomen, even when touching them.” Also symptoms 873 and 878—“A paralytic dragging in the muscles of the thigh and calf; painful when walking.” “Pain, as after a violent exertion, in the muscles of the thigh; they feel as if they had been beaten when touched.” It is worthy of observation, too, that Nux Vomica gives rise to night-sweat of a sour smell. Without building too much on this last symptom, yet, from the beneficial effects of the medicine in certain forms of muscular rheumatism, we may fairly assume that Nux Vomica

exerts a curative influence on the proximate course of this troublesome complaint.

In its simpler forms—as, for example, lumbago or wry neck—I have generally found the treatment satisfactory; indeed, some of the most striking examples of rapid and enduring relief from the agonising pain of several days, or even some weeks' continuance, have been presented by cases of lumbago, which have resisted the usual routine of blisters, &c. At the same time, I must confess that I have met with great difficulty in the successful treatment of chronic muscular rheumatism, especially when there is wasting of the muscles; and I am inclined to believe that Dr. Fuller is in error in attributing this wasting merely to the inaction of the parts. I conceive it is due to the cause of the rheumatism, whatever that may be, preventing the proper nutrition of the affected muscles.

I have now, gentlemen, in these lectures, given examples of the treatment pursued in this hospital of almost every variety of rheumatism; and I leave it to you to decide how far the method of therapeutics here adopted has proved itself equal to cope with this truly formidable disease; and how far we are justified in relinquishing the measures in ordinary use in other establishments devoted to the same great object as this one—the cure and alleviation of human suffering.

LECTURE VI.

ON THE DOSE AND THE ALTERNATION OF MEDICINES.

I POSTPONED the consideration of the methods of administering Homœopathic medicines, that I might treat the subject of the dose with something like the fulness its importance demanded; and now that I come to fulfil my pledge, and to enter upon a question that has caused so much and such warm discussion, I feel the greatest hesitation and reluctance in entering upon the task. It is quite out of my power to give even the briefest outline of controversies, both within and without our own body, which this question has elicited—controversies conducted with much ability on both sides, but not always with perfect candour and equanimity on either side.

Passing these by, for the present at least, let us attempt fairly to understand what the difficulties are which have prevented the same unanimity among the adherents of Hahnemann in reference to the proper dose of Homœopathic medicines, as happily prevails in regard to the fundamental dogma on which his system is erected, and from which the minor proposition, that a substance chosen according to the principle expressed in the maxim, "*Similis similibus curantur*," should, to effect a cure, be given in a quantity infinitely less than practitioners of medicine had been in the habit of employing up to his time. To use his own words, as he expresses himself in the 278th paragraph of the

Organon—"Here the question arises—What is the most suitable degree of minuteness for affording certain and gentle relief?—how small, in other words, must be the dose of each individual medicine (selected according to the Homœopathic principle for a case of disease), in order that it shall best effect a cure? * * * Pure experiments, careful observation, and accurate experience, can alone determine this; and it would be absurd to adduce the large doses of unsuitable (Allopathic) medicines of the old system, which do not touch the diseased part of the organism Homœopathically, but act only on those parts unaffected by disease, in opposition to what pure experience pronounces respecting the requisite minuteness of the doses for effecting Homœopathic cures." The question is here fairly stated to be one which can be correctly answered only by *experiment*, *observation*, and *experience*.

Let us consider how it happens that we have, up to this time, obtained nothing but confused, evasive, and contradictory replies to the interrogations put to this collective oracle.

At the outset of the inquiry, we perceive a fundamental distinction between the processes of investigating the question of the proper dose of an Allopathic and a Homœopathic medicine. In Allopathic practice, the design, as Hahnemann states, is, 'by acting on a healthy organ, to induce a salutary change on the part of the body affected with disease. For example, in a case of rheumatic inflammation of the knees, our Allopathic practitioner might give a

purgative, a drastic, or a sudorific, and opiate. If he is asked how he knows what quantity of any of those drugs ought to be administered, his answer is ready—"That will depend on the amount of effect required." The effects of the substances he employs have been ascertained, with greater or less precision, by scientific processes. It is known to a nicety what quantity of castor-oil will operate as a purgative, and what quantity of opium is required to produce sleep. Making a certain allowance for the disturbing action of morbid causes, he selects his dose of medicines according to the table put into his hands by the toxicologist. For an Allopathic practitioner to give the millionth of a drop of castor-oil to induce purgation, or the billionth of a grain of opium to procure sleep, would be a manifest absurdity. His aim is definite, and the means to attain it are equally so. The question is not now whether or not purgation be desirable; but supposing it to be so, how is it to be obtained? He can graduate his dose so as to produce, to considerable nicety, the amount of the particular action he wishes, because he is acting upon the sound part of the frame, according to physiological principles established on the secure basis of repeated experiment. But the position of the Homœopathic practitioner is wholly different. The effects he strives to obtain are not *positive*, but *negative*. For what is a *cure* but the negation of disease? The more perfect the method, the more entirely absent is all positive effect. This, then, is his problem—"Given thirty different doses, all of which have the power of acting curatively

upon a diseased organ, while none of them produce any appreciable effect upon the sound parts of the body—how is he to ascertain which one out of the thirty exerts a curative power in the strongest degree?” The simplest, and, indeed, the only way to solve this problem is, by a series of trials of the same medicine in different dilutions, administered in *similar* circumstances. But at the very threshold of the investigation, we encounter the almost insurmountable obstacle of contriving experiments which shall meet the requirements of the case. The difficulties that beset the whole class of such inquiries are thus expressed by Mr. J. Stuart Mill, in his great work upon Logic:—“Let the subject of investigation be the conditions of health and disease in the human body—or, for greater simplicity, *the conditions of recovery* from a given disease; and, in order to limit the question still more, let it be confined in the first instance to this one inquiry—Is, or is not, a particular drug—Mercury, for example—a remedy for that disease?
* * * When we devise an experiment to ascertain the effects of a given agent, there are certain precautions which we never, if we can help it, omit. In the first place, we introduce the agent into the midst of a set of circumstances which we have exactly ascertained. It need hardly be remarked how far this condition is from being realised in any case connected with the phenomena of life; how far we are from knowing what are all the circumstances which pre-exist in any instance in which Mercury is administered to a living being. This difficulty, however, though insuperable

in most cases, may not be so in all; there are sometimes (though, I should think, never in physiology) concurrences of many causes in which we yet know accurately what the causes are. But when we have got clear of this obstacle, we encounter another still more serious. In other cases, when we intend to try an experiment, we do not reckon it enough that there be no circumstances in the case the presence of which is unknown to us; we require, also, that none of the circumstances which we do know shall have effects susceptible of being confounded with those of the agent whose properties we wish to study. We take the utmost pains to exclude *all causes capable of composition* with the given cause; or, if forced to let in any such causes, we take care to make them such that we can compute and allow for their influence, so that the effect of the given cause may, after the subtraction of those other effects, be apparent as a residual phenomenon. These precautions are inapplicable to such cases as we are now considering. * * * Anything like a scientific use of the method of experiment in these complicated cases is therefore out of the question. We can, in the most favourable cases, only discover, by a succession of trials, that a certain cause is very often followed by a certain effect.”*

I have quoted this utterance of inexorable logic, not that I believe it to be at all impossible to establish, by a sufficient number of instances, that the administration of a certain remedy is followed, by virtue of

* Mill's *Logic*, vol. i., p. 529.

the law of causation, by the cure of certain forms of morbid action, but to put us on our guard in dealing with the question before us, by exhibiting the category to which the problem belongs. It is one of mixed causes. Without undervaluing the power of a medicine to effect a cure, yet it is obvious that this curative action is only one of several causes which bring about the recovery of a patient. The duty of a physician is not to make experiments upon the sick entrusted to his care, in order to determine, with scientific precision, the exact value of a given remedy; it is, to employ all the means in his power to accomplish the object of his vocation. He must, to the utmost of his ability, remove from his patient all noxious agencies, and accumulate all salubrious influences about him. He must improve his diet, his habits of exercise, or of rest; he must convey hope and confidence, if possible, into his mind; and, in short, he must exhaust all his ingenuity for the welfare of the sufferer, just as if he had no medicine to give. Into this multitude of healing forces he drops his globule. Let it not be supposed that I regard the globule so administered as a trifling addition; on the contrary, I am firmly persuaded that, in most cases, it exerts greater powers in bringing about the happy result than all the other influences put together. Nevertheless, to overlook the other co-operating causes of the effect, would be as great an error on our part, as is the mistake of our opponents in ascribing our cures to these causes alone. Let us now fairly confront the problem. The question is—"Here are thirty different

doses of the same substance—all the thirty possess the power of curing—to ascertain which of the thirty possesses this power in the highest degree?" The only conceivable method of setting about the investigation is to institute a series of experiments. What are the conditions required to give such experiments any scientific value? It is obvious that the physician must begin by selecting thirty cases of the same description in every particular; that the cases must be of some complaint which does not tend to a speedy spontaneous recovery; that, for each of these cases, the medicine experimented on is equally well suited; that all the persons are placed in exactly similar circumstances as regards the other sanative influences; that they are all equally sensitive, naturally, to the action of the particular remedy which is being tried; that the experiment shall be made by the same physician, at the same season of the year, surrounded by the same epidemic and atmospheric influences; and that it shall last for several weeks, or even months. But *one series* of such experiments could not by any possibility afford any trustworthy results. It would require many such series. Now, I ask—Has this ever been attempted, or is it likely that it ever will be? Common sense tells us that the difficulties of performing such experiments are absolutely insurmountable. But, till they are overcome, I do not see how we can expect ever to arrive at a *law* regarding the dose. This can be attained by a process of induction alone; and the only facts available for such induction must be obtained by some such process of experimentation as I

have sketched. All *à priori* attempts to frame such a law have utterly failed.

The practice of giving medicine in extremely minute doses, in cases treated Homœopathically, rests partly upon reason, partly upon experiment. The rationality of the practice was explained sixty years ago, by Hahnemann, and with more force and clearness, in my opinion, than by any subsequent writer on the subject. In a letter addressed to Hufeland, and published in his celebrated Journal in the year 1801, Hahnemann writes—"You ask me what effect can 1-100,000th part of a grain of Belladonna have? The word *can* I dislike; it is apt to lead to misconceptions. Our compendiums have already decided what medicines *can* do when given in certain doses; and they have told us exactly what we are to use: they have determined these matters with such precision, that we might look on these volumes as our sacred books, if medical dogmas were to be believed as articles of faith. But, thank Heaven, this is not the case: it is well known that our works on *Materia Medica* rest on anything but pure experience—that they are, in fact, the inanities of our great-grandfathers, repeated, without examination, by their great-grandsons. Let us not, then, inquire of these compendiums, but let us ask nature what effect has 1-100,000th of a grain of Belladonna? But even in this shape the question is too wide; it must be put in a more definite form for reply, by the addition of the qualifications, *ubi, quo modo, quando, quibus auxiliis*.

“A hard dry pill of extract of Belladonna, produces on a robust, *perfectly healthy* peasant or labourer, usually *no effect*; but, from this, it by no means follows that a grain of this extract would be a proper, or too small a dose, for such a man *if he were ill*, or *if the grain were dissolved*. The most healthy and robust labourer will be affected with very violent and even dangerous symptoms from one grain of the extract of Belladonna, if this grain be dissolved in, say two pounds of water, and if he take it in spoonfuls within six or eight hours. These two pounds will contain about 100,000 drops. Now, if one of these drops be mixed with other 2,000 drops of water, and twenty drops of this mixture be given every two hours, they will produce effects not much less violent if the man to whom they are administered be *ill*. This dose contains about one-millionth part of a grain. A few teaspoonfuls of this mixture will, I affirm, bring to the brink of the grave a person seriously ill of a disease to which the action of Belladonna bears a close resemblance.

“The hard pill of some grains of Belladonna finds few points of contact in the healthy body; it glides almost undissolved through the intestinal canal, protected by its layer of mucus. But it is very different with the same substance when dissolved. Let the solution be as weak as it may, on being received into the stomach it acts on a much larger surface, and excites more severe symptoms than the pill which contains a million times the amount of medicine is capable of doing.”

In this letter, written more than sixty years ago, we have the most explicit statement of the conditions which combine to make the administration of medicines in minute doses obviously rational. These conditions are twofold; the first is, that the substance shall be prepared in such a way as to enable it to come in contact with a large surface of a part of the body highly endowed with nervous sensitiveness. This is a physiological reason. A healthy man would be poisoned by a dose of Belladonna or Opium, if given in solution, which would be comparatively innocuous if given in the form of a pill. The second condition is pathological—viz., that an organ in a morbid state is morbidly alive to morbid agencies—as the inflamed eye to light; the inflamed ear to sound; or inflamed joints to movement.

If Hahnemann had stopped at this point, he might have effected what I may call a bloodless revolution in medicine. But he did not stop here. A few years after writing the passage just quoted, he uttered the astonishing proposition, that a globule of the decillionth of many substances known to be wholly inert in massive doses, had the property of exerting an enormous power upon the living organism when it was afflicted with symptoms of disease similar to those these substances produced when they were prepared and administered in a certain way. It is obvious that, in making such an affirmation, Hahnemann left behind all *à priori* probabilities of its truth. The reasoning he employed to render credible that a millionth of a grain of Belladonna was more potent than the whole

grain, inasmuch as the former acted on a larger surface, is inapplicable in regard to a globule of the 30th dilution of Calcarea for example, placed upon the tongue. Up to a certain point analogy is all in favour of minute doses; beyond that point, I confess it seems to me that we can derive little, if any, support from analogy.

On what, then, do we rely if we entertain the belief that a globule of the decillionth of a grain of Calcarea is a powerful medicine? Simply and exclusively on testimony and our own observations.

Let us consider the evidence in favour of this astonishing proposition. The first witness is Hahnemann himself. Is his testimony worthy of credit in this matter? We require of a trustworthy witness that he shall be competent in knowledge of the subject, and that he shall be veracious and accurate in his statements. Of Hahnemann's acquaintance with the subject, and capacity of observing facts in regard to the action of the high dilutions of medicine, there is no need to speak. He had ample opportunities, and undeniable ability for the task.

But what of his accuracy and trustworthiness as a recounter of observations? If Hahnemann be not accurate and trustworthy as a critical observer and faithful narrator of his own observations and those of others, then the whole fabric of Homœopathy rests on an unsound foundation; for be it remembered, that nine-tenths at least, if not nineteen-twentieths, of the observations upon the actions of medicines which are in daily use by Homœopaths, are derived from

Hahnemann's *Materia Medica*: and observe what an unenviable position those men occupy who are in the daily habit of employing the medicines introduced into practice by Hahnemann—prescribing Belladonna, Pulsatilla, Bryonia, &c., not because they themselves discovered the virtues of these drugs, but because Hahnemann has done so, and left directions for their proper use; and who, although tacitly acknowledging his trustworthiness, yet tell us that they have no faith in globules of the high dilutions! I could quite understand their position if they were to say—We have tried both the low dilutions and the high dilutions, and we find the latter of no avail. But I do not find that those who reject Hahnemann's statements in reference to the actions of the high dilutions, have submitted them to a careful course of experimentation. On the contrary, it seems to me that they act exactly as they blame the opponents of the Homœopathic system for acting—that is, they deny that these doses can act, because it is against their notions of what is possible. But who shall say what is possible and what is impossible? Facts alone, facts carefully observed and accurately reported by witnesses beyond suspicion, can decide such a question as this. Hahnemann, as we know, with nothing to gain and all to lose by a false step, firmly maintained his course, and in cases of the severest illnesses of those most dear to him, trusted in his globules of the 30th dilution. Since his time a multitude of other observers have borne testimony to the efficacy of these extremely minute doses. If we were to poll all the Homœopathic practitioners over

the globe, and put the question to them, "Do you believe that a dose of *Calcarea* of the 30th dilution has any power?" I am convinced that there would be an overwhelming majority in favour of the affirmative.

All who have earnestly devoted themselves to the study of the *Materia Medica*, and to an experimental verification of Hahnemann's original experiments upon himself with the medicines it contains, have borne testimony to the wonderful fidelity and accuracy of all the statements that rest upon his own personal observations. Are we, then, to throw all these observations to the winds, and to begin a reconstruction of this great edifice, which looks rather like the growth of ages than the effort of one man? Those who propose that we shall, have formed a very faint idea of the difficulties that beset a labourer in the path of experiment with medicinal substances; in fact, nobody who practises Homœopathy, can seriously make so wild a proposal, for he stultifies his own practice at the very outset. No; logically there is no alternative. Either we must accept the statement of Hahnemann and his followers, that globules of the decillionth of a grain of such substances as *Calcarea*, *Carbo*, *Silicea*, have a most powerful action on the disordered frame of man, or we must reject Hahnemann's *Materia Medica*, out of which all the manuals of practice are derived. If we are not prepared for the latter alternative, what becomes of the lamentations we so often hear about these high dilutions? If Hahnemann be correct in his statements, then he made, undoubtedly, as great a discovery when he ascertained that there was medi-

cinal power in the decillionth of a grain of lime or charcoal, as was ever made in this region of physics. Shall we say this discovery is of no importance—a thing rather to be ashamed of, because it exposes us, who avow a belief in it, to the ridicule of those who despise Hahnemann, his doctrines, and his disciples? Were we to do so, we should deserve the contempt of his opponents, whom, by denying our master, we ignobly tried to propitiate, in order to avoid the scorn of the medical world.

If, on the one hand, it is impossible to ascertain, by the strictly Baconian method of scientific induction, the relative value of different doses of the same medicine; and, on the other hand, there is ample evidence that all the various doses ranging from the pure tincture up to the decillionth of a drop possess power, more or less, to cure diseases, what is the practical conclusion to which we are forced to arrive? It is this. The dose is not to be determined by any general law; each individual, each form of disease, each variety of the innumerable conditions, the sum total of which compose a human life, has its own appropriate dose. The dose proper for this man to-day will be improper for him to-morrow: the dose best suited for this woman is not the best for that child; but all men are not to have a different dose from all women and all children. The question of the proper dose is like that of the most suitable food. The only answer we can give is—"That is best which agrees best with the individual who uses it." In the same way we say—"That dose is best which does its work most perfectly." But if we

are asked—How shall we determine beforehand what particular dose to select? our answer is—“It is impossible to give any general rule; it depends upon the individual sensitiveness of the patient; and to be able to form an accurate estimate of this, requires the special *medical faculty*, the possession of which, in a greater or less proportion, is the grand reason of the difference in the success of one practitioner as compared to another. The same kind of rapid perception, almost intuition, which enables a skilful physician to form at once an accurate judgment of the nature of a case, will enable such a man also to form a correct opinion as to the amount of sensitiveness, and, consequently, the most appropriate dose. In short, it belongs to the unteachable part of *the art* of medicine, not to the teachable *sciences* which go to form that art.”

The great practical lesson to be learnt from all that has been written on the subject, I believe to be this—That man will effect most cures who, besides being duly instructed in the action of the medicines, and possessing in greatest measure the other qualifications for success, is least bound by any rules in regard to the doses he employs—who ranges most freely from the lowest to the highest, and from the highest to the lowest, and is neither deterred from giving a globule of the 30th by fear of the incredulous, or of giving a drop of a pure tincture out of dread of the purist, and who uses his liberty without infringing that of others. That Hahnemann should have desired to promulgate an act of uniformity in our posology is natural, seeing he was expected to do so as the head of the largest

and most influential school that has arisen since the breaking up of the Galenic empire of medicine; nor do I find it strange that, in his practice, Hahnemann, who, as a successful practitioner, was probably never equalled, should, by the few hints he drops in regard to the doses he employed, show that he was in the habit of employing all doses, from drops of the pure tincture to portions of a single globule of the 30th dilution.

It was natural that Hahnemann, as the founder of a school, should desire to introduce a certain degree of uniformity into the practice of the system he inaugurated, for the obvious reason which he himself gives—that if all used the same dose, we should be better able to form a correct opinion as to the treatment of any case, and be more secure of obtaining the same results in similar circumstances, than if there were a great latitude in the amount of medicine employed; and also, be it remembered, he fixed upon the 30th dilution to put a check to what he regarded as the extravagances of some of his disciples, who soared away into the hundredth, and even thousandth dilution; and, in their devotion to these ghostly sublimities, were in danger of passing into a state of such mystification, as to forget that the dose was, after all, a matter of only secondary importance.

While Hahnemann claimed for the higher dilutions their rightful recognition on the ground of their proved usefulness, it was hardly possible for him to avoid framing some hypothesis to account for the extraordinary development of powers which had never

been before revealed. The explanation he advanced of these surprising powers was, that they were evoked or engendered by processes of trituration and succession; and he termed the result dynamisation. We are not in the habit of scoffing at Newton's discovery of the analysis of the sun's light by the prism, because he appended to it an hypothesis as to the essential nature of light, which is generally disallowed; and it seems to me to argue the opposite of a philosophical character, for any one who believes in the discoveries of Hahnemann, to attempt to hold up his hypothesis to ridicule. If we accept his facts, his explanation is at least as good as any that has hitherto been proposed. How does it happen, he asks, that although the water of this district is highly calcareous, it does not cure diseases for which that substance is suitable, and which are cured by *Calcarea* dynamised by trituration? Here is a question which is yet unanswered; and surely it is more unphilosophical to reject a fact because its discoverer attached an hypothesis to it, than to frame an explanation in accordance with the general theories of the time in which the discoverer lived; which Hahnemann did. That there are latent "spiritual" powers in matter was a prevailing opinion long before the time of Hahnemann. It is thus expressed by Lord Bacon:—"Let this be laid for a foundation, which is most sure—that there is, in every tangible body, a *spirit* or *body pneumatical*, enclosed and covered with the tangible parts."*

For my own part, I am disposed neither to admit

* *Sylva Sylvarum*, p. 696.

nor to deny the sufficiency of Hahnemann's hypothesis. It is perhaps as good as any other, and merits careful consideration at our hands. Still it is only an hypothesis, and does not touch the validity of the evidence in favour of his great discoveries—First, that the true method of curing diseases is to select a remedy which produces in the healthy body symptoms similar to those of the patient whom we are treating; and, secondly, that a remedy so selected has the power of curing even in quantities so minute as one decillionth of a grain or drop.

The second proposition, which we may thus modify, "that medicines have the power of curing in so great a variety of fractions of a drop or grain, that the particular fraction we select, from the 10th to the decillionth, is of comparatively secondary consequence," holds true, however, only under the conditions that we choose a remedy strictly in accordance with the maxim expressed in the first proposition—the Homœopathic formula. If we leave this narrow path traced out by Hahnemann, and attempt to discover specifics by any other plan, then the whole complexion of the question is at once changed. To illustrate my meaning, let me direct your attention to an interesting paper which appeared in a recent number of the *British Journal of Homœopathy*, by Dr. Kidd, on the treatment of fibrous tumours of the uterus. Dr. Kidd observes—"The Homœopathic treatment of fibrous tumours illustrates the necessity we have to treat diseases, not symptomatically, but rationally. No medicine is known to cause the production of fibrous

tumours; and although medicines, such as Sabina, Secale, and Ferrum Muriaticum, are Homœopathic to the symptoms caused by the tumours, yet their use is only palliative, and in no way curative of the disease."

"From the pathogenetic effects of Mercury, it seems to be the nearest Homœopathic specific for the disease. The primary pathogenetic effect of Mercury is to cause an *increase in the quantity of fibrine in the blood*; also an increased activity in the fibrous structures, and in the fibrous organs, such as the womb. In practice I have found it the most useful remedy in the treatment of this disease." Dr. Kidd then mentions that he gives from one to three drops of the second decimal dilution of Corrosive Sublimate two or three times a day; it may be for many months. The second decimal dilution is the 100th of a grain; and three drops of that three times a day, would make a daily quantity of this powerful salt of Mercury taken by the patient about the 11th of a drop. In this passage we have two statements—the one is that of a fact, and a most important fact, coming from a physician known to have had a very extensive experience—viz., that a given quantity of Corrosive Sublimate cures fibrous tumours of the uterus. We have also, in explanation of the fact, the statement that Mercury acts homœopathically, having the power to increase the fibrine of the blood. While gratefully accepting Dr. Kidd's fact as an important addition to our resources, and trusting that, in the hands of others, this remedy may prove as useful as he assures us it has been in his, I am compelled

absolutely to reject his explanation of the *method* of its curative action ; for I find all the authorities on the subject describe the action of Mercury upon the blood as the very reverse of what Dr. Kidd affirms it to be. So far from Mercury producing an excess of fibrine, it causes a marked reduction in the quantity of that ingredient of the blood. Dr. Headland, in his well-known prize essay on the action of medicines, makes the following observations in regard to Mercury :—“ Mercury disintegrates or decomposes the blood, and thus wastes the body. This is the systemic action of Mercury, on which too much stress cannot be laid. Dr. Wright has analysed the blood of patients under mercurial action. It is materially changed : it contains more water, and is more prone to putrefaction, than healthy blood. The *fibrine*, albumen, and red globules, are diminished in amount.” * * * Wibmer, who has collected all the cases of mercurial poisoning which had been published at the time he prepared his wonderfully elaborate work, mentions, as one of the characteristic effects of Mercury, “ the increase of the fluidity and decomposition of the blood ;” and I can find no authority for the statement that Mercury, at any stage of its operation, increases the fibrine. If we are constrained to disallow Dr. Kidd’s explanation, are we, therefore, called upon to reject his facts, and the practical deduction to be drawn from them ? Heaven forbid that we should be possessed of so narrow a spirit ! No. Let us accept these, and all facts which add to our power of coping with the multitudinous forms of disease claiming our efforts, to cure or relieve :

but let us arrange such facts in their proper order, and call them by their proper name. If Mercury have the power of curing fibrous tumours in virtue of its action on the blood, then it is by diminishing the fibrine in that fluid. Now, this action is not pathological, but physiological, and probably chemical; as such it is removed entirely out of the conditions to which the proposition respecting the dose of Homœopathic medicines is applicable. To cure in this way we must use comparatively large quantities, in order that they may produce the physiological effect; we have no right to expect any action at all in this method from the minute doses usually employed by Homœopathic practitioners. In fact, such cures are not Homœopathic in any sense. Let this be distinctly understood, otherwise we shall find that those who attempt to repeat them, and, ignoring this important fact, employ the medicines in such doses as they are themselves in the habit of prescribing, will be grievously disappointed in the results. Let us recognise, as a significant sign of the times, the rise of a form of specific therapeutics, entirely different from Homœopathy, and likely to have a powerful influence upon medicine—beneficial as regards Allopathy, to which it will naturally ally itself, and, in my opinion, detrimental to Homœopathy, which it will deprive of that purity and certainty so remarkably in contrast with the vagueness and uncertainty of all other methods hitherto introduced.

While it may be a cause of legitimate regret to see—as we do in Germany—a large number of men prefer

the guidance of Paracelsus to that of Hahnemann, in their quest for specific remedies, we are bound to concede that, from their point of view, the massive dose is perfectly rational. But we cannot say so much of those who adopt the principles of what I think Dr. Chambers calls conservative or constructive medicine, and attempt to carry them out with infinitesimal doses. For example, there are some diseases in which it is known that iron exists in deficient quantity in the blood. Now, there are two plans, both rational, for treating patients so affected. The one—the constructive—is to administer iron in such a form that it can be assimilated by the invalid, and restore, by its chemical action, the blood to its normal state. That this is possible is proved by the researches of the chemist. The other plan is to discover medicines which shall enable the organism to appropriate for itself the iron which exists in abundance in articles of food. If we adopt the latter plan, then we should employ infinitesimal doses; if the former, we must give the iron in tangible quantities. To prescribe it in the billionths of a grain, is as irrational as it would be to administer the millionth of a drop of tincture of Senna to obtain an evacuation of the bowels.

I now come to the consideration of the question of the administration of two medicines in alternation—a practice which may be said to have become usual, and which is often adopted even by those who condemn it. The short time at our disposal will prevent me from entering with any fulness into this division of the subject; and I shall content myself with making a few

general observations on the advantages and disadvantages of this plan of treatment.

In the first place, it is so manifestly at variance with the doctrines taught by Hahnemann, in regard to the necessity of counteracting one morbid dynamic change, in which he considered a disease to consist, by some one other medicinal dynamic action, and to his belief of the length of time that a single dose of a medicine acted, that he could not possibly regard the plan with anything but repugnance from his theoretical point of view. If disease be one and indivisible, and if its symptoms be but the mutterings of this evil spirit, and if, on the other hand, a medicine act as a whole—giving a shock to this malignant spirit, as knight charged knight in the encounters of chivalry—then, with this idea of morbid and medicinal action, it was obviously inconsistent to administer more than one medicine at a time, or to give a second dose of this until the effects of the first were exhausted. But Hahnemann, besides being a genuine thinker; a profound excogitator of a system derived from certain assumed axioms, was a man of great common sense, of large experience, and of practical sagacity. And in medicine, as in politics, the most successful administrator is he who, although fully recognising the general laws which philosophers have propounded, yet adapts his measures to the special exigencies of the occasion, even although, for a time, they seem to be in violation of his most cherished maxims. In medicine, the law of humanity is the highest of all laws; and Hahnemann showed by his practice, that while recognising the general propositions

he had laid down in the *Organon*, he did not allow them to interfere with his freedom of action in dealing with dangerous disease. In proof of this, we find that when he treated a form of typhus fever, in the year 1814, he administered, in alternation, Bryonia and Rhus Tox. In the edition of his *Materia Medica* published in 1833, he mentions, in the introductory observations to Rhus Tox., that the only remedies which proved effectual, were Rhus given in alternation (abwechself) with Bryonia. Nor does he make any deprecatory observation on the practice. So that even when he strenuously maintained the theoretical badness of this method of procedure, he does not impugn it in an instance in which experience proved it to have been useful. It is but fair to observe, that although he speaks of giving these medicines in alternation, yet, in the fuller history of the epidemic, he describes the cure as usually effected by a single dose, first of the one, and then of the other medicine.

If, however, we reject the notions of Hahnemann, in regard to the essential nature of disease, as incompatible with the modern pathology which has come into existence since he wrote his *Organon*—a pathology not like that against which he inveighs, founded upon conjecture, but the fruit of extensive and accurate observation on the results of morbid changes, and of careful inference therefrom as to the nature of the actions which produced these organic alterations in the body—are we, therefore, justified in also rejecting his practical advice, never to give but one medicine at a time, and never to give a second until the operation of the

first has wholly exhausted itself? There still remains another serious objection to the administration of medicines in alternation; and that is, that it introduces such an element of confusion into the problem of what did good in any particular case, as in some degree to vitiate the scientific value of the result we obtain. If, in a case of acute rheumatism, I prescribe Aconite and Bryonia alternately, and the patient recover, how shall I determine whether the cure were due to the action of the one or of the other, or of a *tertium quid* compounded of both? That this is a most serious objection must be admitted; and the conclusion I am disposed to come to is, that when we adopt the system of alternation, it should be done always under protest. The paramount duty, however, of the physician being to cure, he is bound to employ the measures which seem to him best suited for the particular case under treatment. All other considerations are secondary to this irrevocable and immutable obligation.*

* In a subsequent Lecture on Fever, the reader will find some further observations on this question.

LECTURE VII.

ON EPILEPSY.

ON looking over the tables of the diseases treated in this hospital during the last year, I was struck with the small number of recoveries entered under the heading, "Diseases of the Spinal Marrow and Nerves." Out of twenty-seven cases, only two are returned as cured; ten were much improved; and eight left very much as they entered. The list includes cases of hemiplegia, hysteria, spinal irritation, cerebral affections, spinal disease, mental derangement, and partial paralysis. It does not embrace epilepsy, as epileptic persons are generally able to attend among the out-patients. If it had, the statistical returns might have been somewhat improved. As these at present stand, we perceive that the diseases of the nervous system are really the most formidable class we encounter in our practice; and, unfortunately, this class is probably greatly on the increase in this country, and in every country where there is a perpetual struggle for existence on the part, not only of the working population—whose frames, when they give way, generally suffer from rheumatism or bronchitis—but also equally or more on the part of the upper classes, who live by their brain and nervous system, and who, besides the constant strain under which the strongest often succumb, are exposed to sudden vicissitudes of fortune.

I propose to confine my observations, in this lecture, to the subject of Epilepsy.

It is calculated that there are about 56,000 epileptic persons in England.* Most, if not all of them, are under medical care: thus it happens that all practitioners of any reputation have almost always one or more cases of this disease under treatment. Now, when we consider the extreme severity and the long duration of the complaint; the distress and anxiety it causes to the relatives of the patient; the obscurity and perplexity of its pathology; and the uncertainty in regard to the proper method of treating it—we need not be surprised that it should be a favourite subject for disquisition by medical authors, and that its literature should be very extensive. It would be out of place here to enter upon an examination of what has been written, even recently, by many able and justly celebrated men; but I shall freely use the information their works supply, while endeavouring to afford the material for a satisfactory reply to the questions which are likely to be asked of us when an epileptic patient is proposed to be placed under our care.

The first question which we may be pretty sure will be—"What are the chances of the patient's recovery?" From what? The term epilepsy represents a very complex and varied series of phenomena; and we must, before giving an opinion, ascertain the kind of epilepsy with which the person is afflicted. There are two very distinct forms: the one called, in the language of science, *epilepsia gravior*; the other, *epilepsia mitior*, spoken of by French and some English writers as *le grand* and *le petit mal*, and familiarly described by

* Sieveking, p. 80.

one of the out-patients of this hospital, as "*his fits and his starts.*" Of the true epileptic fit, or falling sickness, there is no need to give a description. When once seen it is impossible to forget. It is not likely to be mistaken for anything else, or anything else for it. Sometimes there is a difficulty in discriminating between certain forms of hysterical convulsions and epilepsy. I believe, however, that a careful history of the case will almost always enable us to decide. As a general rule, hysteria may be safely considered as a peripheral affection of spinal and other nerves; while epilepsy is a central affection of those parts of the nervous system which are included in the encephalon—*i. e.*, the brain proper, the cerebellum, and the medulla oblongata. It is very rarely that we find the mind affected in hysteria; at the same time I have met with cases where there was temporary aberration of reason, along with violent clonic convulsions, attended with unconsciousness, and ending with sleep, and yet which were undoubtedly shown, by the course they took, and the previous history of the patient's complaints, to be of a hysterical kind, and not to be epilepsy. Some writers lay much stress upon the state of the pupil as a diagnostic sign: I have no great faith in this. It is difficult to get a good view of the upturned eye of a person rolling on the ground and struggling violently; and I have observed the pupils, in hysteria, dilate just as much as they are said to do in epilepsy.

The attacks of *epilepsia mitior* present a great variety; sometimes they are simply what the term implies—fits of a kind similar in their character, but

much slighter in degree than the regular fits, lasting, it may be, for one or two minutes; sometimes they appear like what might be described as a transient blush of the brain (as it were): there is a momentary suspension of consciousness, and the slightest possible tremor of the hands, and it is over. But there is another form bearing an exact resemblance to natural somnambulism, or to the condition of persons in that curious state known by the absurd name of electro-biology, which seems nothing more than somnambulism artificially produced. I had for some time under my care a patient afflicted with true epileptic fits, who was, besides, subject to these attacks of somnambulism in the day-time. On one occasion she called upon me at the usual hour for receiving patients, and took her place in the waiting-room. When she was told by my servant that I was disengaged, she rose and walked into my consulting-room, sat down, and answered questions quite coherently, although her manner was somewhat strange and absent. However, I had no idea she was unconscious till she suddenly started, and declared she had dropped a piece of money: this she had certainly not done in my presence, and her purse was in her hand. It was a clasp purse, and shut. On looking in the waiting-room, the money was found on the floor. Immediately after dropping it she had passed into the state—a kind of sleep; her actions were no longer influenced by direct volition, but probably by obscure dream-memory: by this impulse she walked into my room and took her seat, and she answered questions as some

persons do when asleep. The condition is a most curious and interesting one, and well worthy of attentive consideration by psychologists. It is wonderful to find a person who is undoubtedly in a state of unconsciousness, and not responsible for her words or actions, returning coherent replies to the questions put to her. The knowledge of this condition may throw doubts upon the legal and moral responsibility of women in some cases of child murder. At all events, it is well that we should be acquainted with the fact, that persons may move and talk as if they were awake and rational, while all the time they are in a profound unconsciousness; and this suddenly and in the day-time; and in circumstances where the absence of their natural intelligence would not for a moment be suspected by any one unacquainted with their habits, or the nature of the malady under which they laboured.

On the frequency and rarity of these intermediate attacks, and their lightness or severity, the prognosis of any case of epilepsy in a considerable degree depends. It seems to be generally admitted, that the frequent recurrence of these slight attacks is more unfavourable, as regards the chances of recovery, than the severity or even the frequency of the regular fits.

If the epileptic patient be under puberty, and especially if of the female sex, much is hoped for from the change that attends the attainment of that condition. This is a popular belief—I fear it is a popular fallacy. That epilepsy often occurs at that age for the first time is undoubted, and quite in accordance with what we should expect; for, according to the Latin adage, *Capitulus*

brevis epilepsia est; and sexual excitement of any kind produces a violent perturbation of the nervous system, and brings the muscular combinations more directly under the influence of the emotions, and takes them out of the control of the will—thus favouring all convulsive actions. But I never could see any reason, except the deceitful one—that what we wish, that we believe—for expecting an existing epilepsy to be removed by the setting in of puberty; and I confess my own experience is dead against it. On this point I find myself at variance with one of our very few systematic writers—I mean Dr. Laurie. In his well-known and very popular work, he says—“When the disease occurs *before the age of puberty*, or when purely sympathetic [which, by-the-bye, epilepsy never is], it is generally curable, without much difficulty, by means of Homœopathic remedies.” If Dr. Laurie’s conviction of the early curability of those cases of epilepsy which appear about the eighth year, or from that to the twelfth, be derived from a sufficiently large number of cases, he would lay the profession under a serious obligation by publishing the details of their treatment, as, in this matter, his experience is opposed to concurrent medical testimony, and is in accordance with universal popular belief.

In connexion with the influence of the development of puberty, we may glance at the allied question of what is likely to be the effect of marriage upon a person affected with epilepsy. We know that hysteria is often cured by marriage; and that, on the whole, we are safe in expressing an opinion, that the fact of a

woman being hysterical is no barrier to her marriage. Can we say so much of epileptics? This most important question has been fully discussed by many able writers.* It presents itself in various aspects to our attention. First, in the simply medical point of view, thus: whether is it safer for a man affected with epilepsy to undergo marriage and its consequence, or to abstain? That some cases of epilepsy have had their origin in consequence of marriage, is a well-known fact. On the other hand, it is affirmed by many credible authorities, that similar results have followed from enforced abstinence. A curious case of the latter description once came under my own notice. It was as follows:—A gentleman of about twenty-four or twenty-five years of age had his leg dislocated at the hip-joint. The dislocation was not reduced, and the head of the femur had to make a new socket for itself in the ileum. To enable it to do so, the patient was obliged to lie constantly on his back. In other respects he was in the enjoyment of a fair share of health; of a most agreeable disposition; highly cultivated; an only son; and of high worldly expectations. In this situation he became attached and engaged to a lady; and the only hindrance to the match being the state of the limb, he impatiently waited the time when he should be able to stand and walk with crutches. He so far recovered as to do so. The marriage-day was fixed, and the guests were invited; among them one of the most influential noblemen in England, kinsman of the bride. A death in her high circle of relations obliged the postponement

* See Sieveking, p. 140.

of the ceremony for ten days. Two days before that interval had elapsed, the surgeon in attendance (along with myself) on the case, was sent for in great haste. On arriving at the house he found the gentleman in a severe, true fit of epilepsy, in which he expired. There was no *post-mortem* examination; but I had no reason to suspect any other cause of death. There was no previous indication of any affection of the heart. The particulars of the last fatal struggle were detailed to me at the time by the surgeon, who enjoys a high reputation, and who gave it me as his opinion that the death was owing to an epileptic seizure, brought on by the prospect of his approaching marriage, and the excitement and restraint of his sexual desires. As a youth he had been of a very amorous disposition, and his accident had prevented him from indulging it.

Admitting the facts of this case, and the inference, they may be interpreted either for or against the prudence of matrimony in the circumstances. It is quite possible that, had this gentleman married on the day he first intended, he might have escaped his melancholy fate. At the same time, it is also possible that the latent epileptic forces which slumbered in his brain, might have been called out into equally fatal energy by the consummation of the marriage act, and have made his end even more tragical. Thus we are compelled to leave the question as to the propriety of the marriage of an epileptic person for the decision of the parties chiefly interested. All we can say is—it may do good, and it may do harm; and it may do neither good nor harm.

But, if we enter upon the general expediency of the step, we have no difficulty in coming to a conclusion *against it*, especially if the epileptic be a woman. On this point we have a clear voice of warning, furnished by statistics; for we find that, out of 110 epileptic persons, there were thirty-one, or nearly one-third, who had epileptic parents or near relatives. Taking another view, we find that fourteen epileptic women gave birth to fifty-eight children: of the fifty-eight, thirty-seven had died under fourteen years of age; and almost every one of these thirty-seven had died of some convulsive disease. With such facts before us, it may be our duty to warn any epileptic who asks our advice on the question of marriage, of the dreadful consequences likely to be entailed upon the offspring, if the marriage be fruitful. If, however, from the age of the parties, or any other cause, there is no chance of offspring, this would materially modify our opinion as to the expediency of the step.

When we are asked to give our opinion as to the probable course and termination of a case of epilepsy, it is likely that we shall be pressed to say whether there is great danger of its destroying the mind; and, on this point, we should be well prepared to give exact information. The prevailing belief, both popular and professional, is, that epilepsy is very apt to end in idiocy, or some other form of insanity. Now, there is no doubt of the close connexion of the two conditions; but Dr. Radcliffe very pertinently observes, that epilepsy *as often begins as ends in madness*—that is, the condition of the brain which induces the latter

condition favours the development of the former ; and thus we are apt to be misled, and to take too unfavourable a view of the prospects of an epileptic patient. The fact is, that the subjects of epilepsy fell into the hands of what are popularly called the mad-doctors. They found their epileptic patients going on from bad to worse, till they became perfect idiots ; and this they ascribed to the epileptic attacks ; whereas the chances are, that the fits of these poor creatures were owing to the causes of their fatuity. A more careful study of the matter has led to a different conclusion ; and the result is, that we have one of the most recent writers on epilepsy giving, as the result of his induction, that "the duration of epilepsy is, *per se*, without influence upon the mental condition of the epileptic."*

So far as my own experience goes, it entirely confirms this opinion. I have had the opportunity of observing a considerable number of epileptics, as I happened to have had one or two very striking recoveries in my practice a good many years ago ; and, in consequence, there was quite a rush of this class of patients. At that time, I had the impression that there was a progressive deterioration of the powers of the mind as a rule ; but I have not found it so. I have watched a good many of these patients ; and, I am sorry to say, that in many of them I have not seen improvement. Still, although the fits have been as frequent, and as severe as they ever were, yet the mind, if it has not developed, has not retrograded. However, I have seldom observed the natural develop-

* Russell Reynolds, p. 173.

ment to go on. Epilepsy seems to blight its powers of growth ; to arrest the mind in the state it was in ; but *frequently* to do no more. Out of sixty-four cases of *true* epilepsy observed by Dr. Reynolds, in twenty-four, or above 38 per cent., the intellect was wholly unimpaired ; in twenty, or above 32 per cent., there was only slightly impaired memory for recent events ; in nine, or about 15 per cent., the apprehension, as well as the memory, was impaired ; and in nine there were general confusion of ideas, amounting to imbecility. If the whole number—sixty-four—be too small to permit us to accept of the propositions exhibited by these figures as altogether trustworthy, they are large enough to prevent our accepting the general notion that epilepsy, if unchecked, passes, as a rule, into mental imbecility. In quoting the tables, I emphasized the *true* prefixed to the word epilepsy ; for, probably, one main cause of what seems to have been an erroneous impression in regard to the effect of epilepsy upon the mind, was confounding this disease with cases of epileptiform affections depending upon tumours of the brain ; in which disease, along with the most dreadful convulsions, we have utter prostration of the mental powers.

Another favourite fallacy is, that much may be done by attending to the general health. Now this requires special attention. If there be any such derangement of the health as is, or may be, an exciting cause of the paroxysms—*e. g.*, intestinal worms—then we may reasonably hope that, with their removal, there will be a cessation of the fits. But even here we must not be too sanguine. We must remember that “fits,”

entirely caused by worms or by teething, are not *epilepsy*; that they are merely peripheral irritation, exciting an action on a comparatively healthy, central nervous system; which action subsides when this irritation ceases. But intestinal worms may be present in a person predisposed to epilepsy, and be the exciting cause of the first attack. If this be the case, we have no reason to expect to effect a cure of the epilepsy by merely removing the worms. *Any* excitement may rouse the latent epileptic condition into active manifestation. For example—"I have seen," says Van Swieten, "a very healthy girl of ten years of age, born of sound parents, who never had epilepsy, rendered epileptic for several years; and the first time she was seized was upon having her soles tickled by some girls who were at play with her, some of them holding her fast upon the floor to prevent her avoiding the intolerable sensation." Now, as school-girls have tickled school-girls, and school-boys school-boys, from time immemorial, and this is the only case on record of such tickling having given rise to epilepsy, we may unhesitatingly conclude, that although, in one sense, the tickling caused the epilepsy, yet it would not have done so had not all the materials for explosion been ready to ignite upon the application to them of *any* spark; and that if, instead of being tickled, this girl had eaten too many raw apples, or any other indigestible food, or if worms had been generated in her intestines, she would equally have had her epilepsy. It is too late now to attempt to comfort her companions, who, from the way the case is told, must have gone

down to their graves a century ago, with this sin upon their conscience ; but it is not too late to point out the absurdity of promoting a mere accidental exciting cause into a primary agent in the production of this disease. And if we, on finding that a paroxysm of epilepsy was first caused in any given case by worms, at once jump at the conclusion that all we have to do is to remove the worms, we shall commit a grave error, and may give rise to unwarrantable hopes and bitter disappointment.

While, on the one hand, we cannot always give security against the recurrence of the fits of epilepsy, by removing the exciting cause which originally induced the paroxysms—as, for example, irritation of the gums from teething—on the other hand, the attacks of the disease may be held in abeyance, for an indefinite length of time, by arresting the propagation of the irritation from the circumference to the centre. Dr. Brown-Séquard, in his treatise on Epilepsy, quotes from Odier a striking case in point. A man had frequent cramps in the little finger of his left hand. The contractions went on increasing in extent and frequency; they, by degrees, extended to the fore-arm, the arm, and the shoulder, always beginning in the little finger. At last they arrived at the head, and then true fits of epilepsy, with loss of consciousness, took place. By means of two peculiar ligatures found the arm and the fore-arm, and which the man could tie easily when he felt contraction of the little finger, the attacks were prevented at every threatening for two or three years. Unfortunately, one day he ate and drank too much ;

and being intoxicated, he forgot the ligaturé when the initial cramp appeared, and then he had a violent fit. From this time the ligature had no more influence over the fits; they became very frequent, and always began in the little finger. Paralysis came on, and the patient died in coma. On examining the head of this patient, an enormous tumour was found in the left side of the brain, below a place where the cranium had been wounded long before. "This case," adds Dr. Brown-Séquard, "and the facts observed in my animals" [*i. e.*, the animals on which he had experimented], "positively show that the apparent outside origin of epileptic fits does not prove that there is not an organic cause in the nervous centres."

A somewhat similar case is related by Dr. Reynolds, who once witnessed, and himself arrested, the invasion of the epileptic force. "The attacks," he writes, "are always preceded by a stabbing pain in the lower part of the belly of the left biceps muscle, on the inner side especially. The pain is not in the skin, but deeper, and seems to go through the arm. If this joint is immediately grasped, so as to press both skin and muscle against the bone, the pain ceases in a few seconds, and nothing more occurs. If pressure is not exerted directly, the biceps contracts and draws up the fore-arm, and it requires firmer grasping and drawing up of the fore-arm to prevent the attack. The pressure must be exerted on the biceps; the effect is not produced by directing it upon the trunks of the nerves, or upon the blood-vessels. Once, while a fit was arrested with my own hand, I observed both radial

and ulnar^{*} arteries to be still pulsating. There is no doubt about the fact that this pressure does, in some way or other, arrest the attacks; it effects more than a relief of pain, which might or might not run on into a paroxysm. This I had occasion to establish once by an attack coming on while I was talking to the patient. He said, 'Here it comes,' and his face betrayed great horror; his respiration ceased, and his pupils dilated widely. I grasped the arm firmly, and the natural expression of face returned; the pupils contracted; the face flushed; perspiration broke out, and nothing more occurred. He did not lose his consciousness. The fits, when, as he expresses it, 'they get past the arm,' are fully-developed paroxysms of *epilepsia gravior*."*

These two very instructive cases demonstrate the possibility of keeping even the worst class of cases of epilepsy in a state of abeyance, if we can discover what gives the initiative to the paroxysm, and cut this off, so that it does not get into the interior, as it were. We gather, too, from these histories, the lesson of the necessity of a very careful investigation of all the steps of the process of the complex series of phenomena called "a fit;" and if we see our way, either by mechanical contrivances or by medicine, to get hold of the first link in the chain, then we become masters of the situation, and may, without imprudence, hold out a fair hope of averting the dreadful consequences that follow, if the evil is not arrested at its origin.

I had lately under my charge a patient suffering

* *Op. Cit.*, p. 94.

under *epilepsia gravior*, the first symptom in whose case was a fulness of the veins of the back of the neck. I never had myself an opportunity of observing this, but it was described to me by the mother of the patient, a person of education and intelligence; and she informed me, that the only treatment that had done her daughter any good, during the sixteen years under which she had suffered from the disease, was a course of medical rubbing by Mr. Beveridge, of Edinburgh. In this case we may presume that the exciting cause of the attack was venous congestion of the brain. We know, from the experiments of Sir Astley Cooper, that interfering with the proper circulation within the head will produce *convulsions*. These experiments are so important, that it may be as well to advert more fully to them. He states that, having tied the carotid arteries of a rabbit, respiration was somewhat quickened, and the heart's action increased, but no other effect produced. In five minutes the vertebral arteries were compressed with the thumb, the trachea being completely excluded. Respiration almost directly stopped; convulsive struggles succeeded; the animal lost consciousness, and appeared dead: the pressure was removed, and it recovered with a convulsive inspiration. It lay upon its side, making violent convulsive efforts; breathed laboriously, and its heart beat rapidly: in two hours it had recovered, but its inspiration was laborious. The vertebrals were compressed a second time; respiration stopped; then succeeded convulsive struggles, loss of motion, and apparent death; when let loose its natural functions returned with a loud

inspiration, and with breathing excessively laboured. In four hours it was moving about, and ate some greens. In five hours the vertebral arteries were compressed a third time, and with the same effect. In seven hours it was cleaning its face with its paws. In nine hours the vertebral arteries were compressed for the fourth time, and with the same effect upon the respiration : after thirteen hours it was lively. In twenty-four hours the vertebrals were compressed for the fifth time, with the same result—viz., suspended respiration, convulsions, loss of motion, and unconsciousness. After forty-eight hours, for the sixth time, the same results were obtained by pressure. Thus it appears, if the carotids are tied, that simple compression of the vertebrals puts an entire stop to the functions of the brain. The experiment was reversed, the vertebrals tied, and the carotids compressed, with similar results. Tying the vertebrals caused the breathing to become laborious; the animal's right ear fell, and the right fore-leg was partially paralysed : in five hours it ran about. The following day, when the carotids were compressed, it fell on its side, losing all sensation and volition, and recovered on withdrawal of pressure. The same results were repeatedly obtained. When both vertebral and carotid arteries were tied at the same time, the animal breathed no more ; but there were from thirteen to fourteen convulsive contractions of the diaphragm, and convulsions of the hinder extremities, and the animal ceased to exist.*

Although these experiments by no means prove

* *Guy's Hospital Reports*, vol. i., p. 457. Sieveking, *Op. Cit.*, p. 196.

that venous cerebral congestion, or a defective supply of arterial blood, is the cause of epilepsy, yet they establish, beyond a doubt, that such a condition of the brain excites convulsions; and if the person in whom this congestion takes place be of an epileptic habit, then there can be no question that, in such a person, it will give rise to true epileptic paroxysms, and that, upon the relief of the congestion, the immunity from attacks will in a great measure depend.

In giving an opinion as to the danger to life, our attention should be directed to the state of the tongue. If this presents marks of having been bitten, it is affirmed, by Schröder van der Kolk, that there is less risk of a fatal termination of the disease. The reason he assigns is this—The most danger to life is from violent irritation of the par vagum. Now he found that, in patients who had habitually bitten their tongue, the capillary vessels in the course of the hypoglossal nucleus in the medulla oblongata, were of a greater proportional size, as compared with those in the track of the par vagum, than in epileptic patients who had not been in the habit of biting their tongue. The opinions of this celebrated anatomist upon this point, which seem to have been suggested by his observations on the morbid structure, have received confirmation from the experience of practical observers; and we may accept them as so far established, and assign to the presence or absence of this symptom, a place among the facts on which we form a judgment of the probability of a sudden fatal termination of any given case.

It may be well here to enter somewhat more fully into the pathology of epilepsy, as, undoubtedly, the subject has been more successfully investigated in recent times; and if we have not arrived at a solution of all the difficulties, we have, at all events, received some useful hints for the direction of our curative efforts.

Dr. Schröder van der Kolk regards the medulla oblongata as the centre of general reflex actions, and, of course, as the starting-point of epileptic and other convulsive diseases. He considers that, however remote the seat of the primary irritation may be, that in the medulla oblongata is the mine which is always fired, wherever the train be led from. Hence, he has directed his special attention to the investigation of the minute morbid anatomy of this portion of the nervous system. The general results of his observations are, that in all dissections of the medulla oblongata in epileptics, whether or not death took place during a fit, he met with great redness and vascular tension in the fourth ventricle, penetrating into the medulla oblongata, sometimes to a considerable depth. Transverse sections through the whole medulla oblongata, from beneath the *pons variolii* to the inferior extremity of the *corpora olivaria*, exhibited the part in the vicinity of the fourth ventricle of a much darker colour, usually containing some more distended vessels, which then ran either in the course of the roots of the hypoglossus into the *corpora olivaria*, or in the course of the vagus and accessory, or in both. Where the degree of redness was slighter, it was commonly confined to the posterior half of the

medulla: in most cases, however, this hyperæmia extended into the corpora olivaria, which were often furnished with large blood-vessels. Thus, also, in the raphé, dilated blood-vessels were almost always visible. After Schröder van der Kolk had discovered the close connexion between the corpora olivaria and the hypoglossus nucleus, he found dilated blood-vessels exactly in this course in the first epileptic patient, whose brain he subjected to a microscopic examination. On measuring the width of the vessels under the microscope, the widest vessels in the course of the hypoglossus were found to amount to 0.230th (two hundred and thirty thousandth) part of a millimètre; in the corpus olivare, to 0.305 millimètre; in the vagus, to 0.152 millimètre. He connected this preponderance of the diameter of the capillaries in the course of the hypoglossus over that of the vessels in the track of the vagus, with the fact that the patient had invariably bitten his tongue in the fits. On the other hand, he discovered that in a patient who had never bitten his tongue, but in whom the respiration was generally disturbed, the vessels in the course of the vagus were much larger than those in the course of the hypoglossus. Hence, the inference we have just noticed, that inasmuch as the functions presided over by the vagus are of more importance to life than those under the direction of the hypoglossus, so there is the greater risk in proportion as the former, and lesser risk in proportion as the latter, are those chiefly affected in any given case of epilepsy. However, it by no means follows as an absolute rule, that in cases in which the

hypoglossus is much affected, the vagus is comparatively little so; they may both be equally diseased; and in such a case, of course, the biting of the tongue would give no security; the respiration would still be in as much danger of being seriously affected, and compromising the life of the patient.

The extreme difference between the diameter of the blood-vessels of the hypoglossus in a healthy subject, and an epileptic, is as 0.306 to 0.096—*i. e.*, as the 306th part of a thousandth of a millimètre to the 96th part of a thousand; in the vagus, as the 0.237 is to 0.111. Upon such infinitesimal differences depend health, with all its enjoyments; and epilepsy, with all its privations and miseries! If this slight dilatation of the capillary vessels were in a part exposed to view—the eye, for example—how easily would they be controlled by applying to the part some remedial agent, whose specific operation took effect upon these vessels. Could we succeed in discovering some substance whose action upon the vessels of the medulla oblongata is equally determinate, we might then indulge the hope of *curing* epilepsy.

Before entering upon the consideration of how we are to do so, and what substances hold out the best prospect of being useful, it will be well to give the analysis of the pathological changes in a fit of epilepsy, as these have been suggested by Dr. Brown-Séquard. He has arranged them in such a way as to give a tabular view of what he considers to be the causes of the various phenomena observed in a paroxysm of epilepsy.

1. Excitation of certain parts of the excito-motory side of the nervous system—i. e., any irritation by tickling, worms, &c.
2. Contraction of the blood-vessels of the face.
3. Contraction of the blood-vessels of the brain proper.
4. Extension of the excitation of the excito-motory side of the nervous system.
5. Tonic contraction of the laryngeal and of the expiratory muscles.
6. Farther extension of the excitation of the excito-motory side of the nervous system.
7. Loss of consciousness and tonic contraction of the trunk and limbs.
8. Laryngismus trachæismus, and the fixed state of expiration of the chest.
9. Insufficient oxygenation of the blood, from many causes; e.g., the little oxygen absorbed; detention of the venous blood in the nervous centres, &c.
1. The effect of this is contraction of the blood-vessels of the brain proper, and of the face, and tonic spasm of some of the muscles of the eye and face.
2. Paleness of the face.
3. Loss of consciousness and accumulation of blood in the base of the encephalon and in the spinal cord.
4. Tonic contraction of the laryngeal, the cervical, and the expiratory muscles. Laryngismus trachæismus.
5. Cry.
6. Tonic contraction extending to most of the muscles of the trunk and limbs.
7. Fall.
8. Insufficient oxygenation of the blood, and general obstacle to the entrance of venous blood into the chest, and special obstacle to the return from the head and spinal canal.
9. Asphyxia.

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| 10. Asphyxia, and perhaps a mechanical excitation of the base of the encephalon. | 10. Clonic convulsions everywhere—of the bladder; of the uterus; erection; ejaculation; increase of many secretions; efforts at inspiration. |
| 11. Exhaustion of nervous power generally, and of reflex excitability particularly, except for respiration. Return of regular inspirations and expirations. | 11. Cessation of the fit; coma or fatigue; headache; sleep. |

It may be well, after this very ingenious analysis of an epileptic seizure, as explained by Dr. Brown-Séquard, to consider how the same phenomena are explained by Professor Schröder van der Kolk. "I think," he says, "we have sufficient reason to conclude that the first cause of epilepsy consists in an exalted sensibility and excitability of the medulla oblongata, rendering the latter liable to discharge itself, on the application of several irritants, which excite it in involuntary reflex movements. The irritation may either be external (irritation of the trigeminus), an irritated condition of the brain, or, as is still more frequent, it may proceed from irritants in the intestines. Acidity, a torpid state of the bowels, &c., are among the most common causes; in adults, there may be irritation of the intestines, particularly of the mucous membranes, constipation and enlargement of the colon connected therewith; but, above all, onanism, which acts so very much on the medulla oblongata, and must be regarded as a very frequent cause

of epilepsy. Amenorrhœa, chlorosis, plethora of the uterus, hysteria, &c., are also to be enumerated.

“In the commencement, there is still only exalted sensibility. If this can be removed or moderated, the epilepsy gives way of itself, especially if the sensibility (*i. e.*, over-sensitiveness) is not renewed by remote causes. But if the disease has already lasted long, organic vascular dilatation (as before described) takes place in the medulla oblongata; the consequence of this being that too great a supply of blood is detained there, and the ganglionic groups are too strongly irritated, too quickly overcharged. Every attack then becomes a renewed cause of a subsequent attack, as the vascular dilatation is promoted afresh by every fit. Lastly, increased exudation of albumen ensues from the now constantly distended vessels, whose walls, at the same time, become thickened, producing increased hardness of the medulla, subsequently passing into fatty degeneration and softening, thus rendering the patient incurable.”

Although, as we perceive, these two eminent pathologists do not entirely agree as to all the causes of epilepsy, yet there is a considerable correspondence in their views as to the different steps of the process which constitute a paroxysm; and we may fairly assume that, if we have not arrived at the whole truth, we are, at all events, approaching the solution of what has been long regarded as one of the most inscrutable maladies which afflict our race, called, of old, the *morbus sacer*, as being a direct manifestation of demoniac power, and requiring nothing short of

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miraculous intervention to deliver its miserable subject from the mysterious and malignant influence which held it in possession. Now that the mystery is so far solved, we may hope that more enlightened therapeutics may enable us to achieve greater triumphs over this terrible malady. But I must reserve for my next lecture a consideration of this part of the subject.

LECTURE VIII.

EPILEPSY (CONTINUED).

I PROPOSE, in this lecture, to confine my observations to the treatment of epilepsy. The success hitherto obtained by the most careful and scientific practitioners, according to the old-school method, has been very unsatisfactory. Out of 115 cases of chronic convulsive diseases treated by Dr. Reynolds—all of which, in common parlance, would have been entitled Epilepsy, and had been treated as such for many years—twenty-one, or 18 per cent., were cured. Of these, however, only eighty were true epilepsy; and the number of recoveries out of these eighty were only eight, or 10 per cent. Out of 191 cases treated in this hospital, thirty-eight are reported as cured, or 20 per cent. But as these cases are chiefly among the out-patients, there is always considerable uncertainty in regard to the results in a disease which may be long dormant without being radically cured. And under the head Epilepsy, it is not improbable that other forms of convulsive diseases may have been included, as in Dr. Reynolds' first list. There is no great difference in the results, judging by these tables, of the old and new system. At all events, whether we excel our neighbours or not, we have no ground to boast, so long as we have to record the mortifying fact, that four-fifths of the epileptic patients who have been treated by us have not been cured; and if any one can discover more certain indications for the

remedies most commonly used, or suggest any means not hitherto employed, and which hold out any prospect of advantage, he will be hailed as a benefactor of his kind.

In considering the treatment of epilepsy, the subject naturally divides itself into general rules for the guidance of patients, so that they may be fortified against the consequences of the disease, and the special indications for the administration of remedies.

In regard to the first, or the general management of epileptic patients, there is a great difference of opinion as to the proper food; one party strongly advocating a low diet, excluding animal food, and forbidding all stimulants; while another party insists upon what is called a generous fare and a liberal allowance of wine. Those who adopt the latter view, argue that we are apt to be deceived into the erroneous idea of spasms being a manifestation of excessive nervous force; while, on the contrary, their presence invariably indicates an enfeebled condition of the nervous system;—that we meet with them as consequences of loss of blood and of impaired vigour generally; and that the restlessness of a nervous patient is not from too much, but from too little life. Especially it is pointed out, that the scanty vitality of epileptics is betrayed by the general coldness of the extremities, and the small, quick, and jerking pulse; and that it is in consequence of this low condition of what we may call the power of the *Anima*, that persons afflicted with epilepsy become subject to cosmical influence; that, in fact, they approach the life of

plants as they recede from that of man. "The plant," says Dr. Radcliffe,* "exhibits plainer and more numerous evidences of periodicity than the animal; and it does this, I argue, because it has less of the inanimate life which enables man and the higher animals to be partially independent of the sun and other vivifying influences which act upon them from without; and hence it follows (this among other reasons), that the man who exhibits more evidences of periodicity than he ought to do, has been shorn of some of that innate life which is the badge of distinction between him and the plant." Whether or not the want of the power of the *Anima* brings the epileptic under the influence of the moon, has been keenly debated; and some of our most recent and most scientific writers declare themselves on the side of the moon. "Although here and there," observes Romberg,† "doubts have been raised against this view, the accurate observations of others have established its correctness." Among these observers a prominent place is always given to Dr. Mead, who, in the following words, describes a celebrated case of lunar influence:—"No greater consent in such cases was, perhaps, ever observed than what I saw many years since, in a child about five years old, in which the convulsions were so strong and frequent, that life was almost despaired of. * * * The girl, who was of a lusty, full habit of body, continued well for a few

* *Epilepsy and other Convulsive Affections.* By Charles Bland Radcliffe, M.D. 2nd ed., 1858; p. 142.

† Vol. ii., p. 205.

days, but was, at full moon, again seized with a most violent fit; after which the disease kept its period constant and regular with the tides. She lay always speechless during the whole time of flood, and recovered upon the ebb. The father, who lived by the Thames side, and did business upon the river, observed these returns to be so punctual, that not only when coming home, he knew how the child was before he saw it; but in the night has risen to his employ, being warned by her cries, when coming out of the fit, of the turning of the water. This continued fourteen days—that is, to the next change of the moon.” This case is generally quoted as demonstrative proof of the moon’s power. Perhaps it would be more correct to accept it as evidence of general cosmical influence; for it may have been, that it was not any direct effect of the moon upon the nervous system of this child, but of the ebbing and flowing of the waters of the Thames.

How much we are all under cosmical influences, is, every now and then, shown when an earthquake happens, or a new epidemic sweeps over a tract of a country, cutting down, as with a scythe, all who have not enough of vital power in them to resist its fatal force. The weak perish; but all, even the strongest, are affected. The most *sensitive* are aware of it at the greatest distance. A curious illustration of this is mentioned in Eckermann’s *Conversations with the Poet Goëthe*, who, although one of the most sensitive, was, at the same time, one of the most robust of men—a great example of that rare nature which, like our own Shakespeare, felt everything, and was subdued by

nothing. One night Goëthe rang for his servant, about midnight; when the servant went, he found that Goëthe had moved his bed to the window, and was gazing upon the heavens. Goëthe asked him if he had seen nothing remarkable in the sky: on receiving an answer in the negative, he desired the man to enquire of the watchmen if they had. They had not. On his servant's return, he found his master still in the same position, and he made this remarkable announcement: "Listen," said Goëthe; "this is an important moment: there is now an earthquake, or one just going to take place." Next day he mentioned at court (Weimar) his conviction, and the duke believed he was right, from his knowledge of Goëthe's character. Some weeks afterwards, the intelligence arrived at Weimar, that upon that night the great earthquake had taken place at Messina, which had overthrown a great part of that city. Here we have an example of an impressionable man perceiving a telluric influence unfelt by his fellows. He perceived it, but merely as a sensation, from which he drew his inferences; for his nervous system was in a state of health. But if it had been in an epileptic condition, would this impression not have most likely given rise to a fit? We have positive proof afforded by the experiments of Dr. Brown-Séquard, that while, on the one hand, an injury of the centre of the nervous system is a predisposing cause of epilepsy; on the other hand, the exciting cause is an impression on the extremities of the nerves in communication with this morbid centre; and that so long, as this part of the periphery is not affected,

the epilepsy may slumber for almost any length of time.

The most important general indication in the treatment of epilepsy, is the knowledge of how, in each case, the strength of the patient can be best sustained. Some thrive best on almost purely animal diet; some on merely vegetable; others on a mixture of the two. We must find out, by careful investigation, which agrees best, and that we must order. The same rule holds good as regards stimulants: to some they are injurious; to some beneficial; and to others indifferent. The great error seems to be, laying down any general rule for *epileptic* patients. There is, and can be, no such rule. Each case must be treated on its own merits, and diet should be as specific as treatment. It is a consequence of vague generalisation, that it is the fashion to order iron, and so-called tonics, in epilepsy. The practice, although recommended by so high an authority as Dr. Watson, is emphatically condemned by Dr. Brown-Séquard. The action of iron on the brain he considers injurious. The same rule, or rather the same latitude and absence of rule, which directs us best in regard to diet, would be good in respect of exercise, and, indeed, of every one of the conditions of health. In a case of epilepsy, we must carefully examine into all the habits of the patient, and insist upon the avoidance of everything which can either damage or enfeeble the general health, and tend to give special animation to any exciting cause of a paroxysm. Above all things, we should have the patient avoid all sources of irritation of the surface

of the body ; being taught, by physiology, how the sensitiveness of the peripheral nerves is exalted by the epileptic condition; and it is rational to expect benefit from soothing ablution with cold or hot water, and the application of soap or oil to the surface of the body. The effect of soap-water—a common expedient in the water-cure establishments—in relieving an over-sensitive condition of the skin, is most marked, and may be of great use in the treatment of epilepsy. What an epileptic wants is strength within and hardness without. The great source of inward strength is food and exercise ; and of outward, hardening friction and proper baths. If we can discover any particular spot where the aura (if there be an aura) takes its rise, we may be tempted to try the effect of a local anæsthetic upon it. The best is probably that recommended by Dr. Brown-Séquard, and consists of half a grain of Sulphate of Morphia, and one-sixtieth of Sulphate of Atropia, and a minim of dilute Sulphuric Acid, in fifteen minims of water. This is to be injected under the skin of the part where the aura originates.

It is now clearly established, that weakness of a muscle, or set of muscles, predisposes it to be affected with spasmodic action. Dr. Brown-Séquard, in a lecture recently delivered in this neighbourhood, and reported in the *Medical Times and Gazette*, of March 28th, observed, that “of two muscles, one atrophied and one healthy, the former will respond to a certain stimulant, while the latter will not ; a weak person will jump or start on hearing a noise,

which produces no effect upon a strong one." This he attributes, not to "weakness of nerves," as it is called, but to the weakness of the muscles. Epileptics are usually very weak, often partially paralysed; their reflex excitability is augmented, while their voluntary muscular power is diminished. Here, then, we have one of the most important of the general indications for treatment—viz., the adoption of means to increase the voluntary power of the muscles. Nothing is better for this than gentle drilling, or a course of what is called medical gymnastics. I have known cases of epilepsy very much benefited by the treatment known by the name of "the Movement Cure."

To sum up, an epileptic patient should be nourished with the greatest care, so as to bring the whole body into the highest condition; but especial attention should be paid to the development of the muscular system; and this should be exercised in such a way as to improve the control over the limbs. Thus, dancing, marching, and all movements which tend to curb the loose shambling gait of the epileptic, are of importance to his cure. Besides, great care should be bestowed in bringing the skin into a healthy state, by baths and by friction, so as to allay all morbid sensitiveness, which is apt to be the starting-point of the train of mischief, which ends in a paroxysm. These are the obvious suggestions made by common sense upon the facts ascertained by modern physiology in regard to the causes of epilepsy; at the same time it is right that we should bear in mind that we shall often meet with epileptics who are in perfect bodily

health, and well developed. Dr. Reynolds lays down as inferences from a large series of observations:—

That epilepsy is not incompatible with perfect physical health.

That it is the exception, not the rule, to find serious impairment of the organic constitution.

That the co-existence of epilepsy with extremely robust health, is more common than the converse.

How far Dr. Reynolds' observations justify such sweeping conclusions I cannot say; but certainly several of the cases that have come under my own treatment, have been persons in whom I never should have suspected any infirmity of any kind, judging from their appearance; and in this class of patients we cannot expect to do much good by any general, dietetic, or regimental management, and must confine our expectations to producing a change in the system of the patient by a steady course of medical treatment.

The treatment of epilepsy by medicines naturally divides itself into those which are best adapted to arrest the first stage, and those which tend to mitigate the paroxysms, and prolong the interval between the attacks.

There seems no doubt that the first stage of an epileptic seizure may be arrested; just as the first stage of cholera is stopped by Camphor. I have witnessed this in cholera. I once saw a little girl of about eight years of age literally *take cholera*. She was in a room where there were two patients in a state of collapse. She suddenly gave a slight cry, and, on looking at her face, I saw the unmistakable but indescribable change

which indicates the invasion of cholera. This is a well-known fact in regard to cholera. In the instance referred to, I immediately gave the patient a dose of Camphor, which I had in my hand. The effect was instantaneous. I watched the life returning into the face, which before had been the countenance of a corpse. It came back slowly and steadily ; the pulse was extremely rapid and small ; it increased in volume and abated in speed, and in about ten minutes the danger of death was passed. The same rapid arrest of an epileptic paroxysm is sometimes effected. "Once," says Dr. Reynolds, "when I was talking to an epileptic, and observing his eye, a fit commenced ; the eyes rolled upwards, and to one side, and the pupils dilated. He had, however, *after* this dilatation, sufficient power to say, 'I am going to be ill ;' but not till then did the distortion begin." This attack was stopped by Chloroform. Similar attacks in other patients have been arrested by placing Ammonia near the nostrils. But neither Ammonia, nor Chloroform, nor anything else, has the slightest effect after the first moment of the paroxysm.

This first stage of epilepsy is probably caused by contraction of the blood-vessels of the brain proper, and of the face, and tonic spasm of the muscles of the eye and face. The effect of Ammonia and Chloroform upon this spasm is very much like that of the smoke of Stramonium upon the asthmatic spasm, and immediate relief is the consequence. Considering that upon the arrest of this first stage so much depends, and that in some respects it is so easily managed, it

seems singular that it should so seldom be effected. The reason is, that this stage is so very short, lasting not above a few seconds; and that even these few seconds have somewhat deprived the epileptic person of his power of thought and action. It is just possible that there may have been real virtue in some of the amulets that were so highly prized even by the least superstitious of the ancient physicians in the treatment of epilepsy. Possibly certain substances worn round the neck, so as to give off their fragrant or pungent particles in the immediate neighbourhood of the extremities of the branches of the nerves that supply the lips and nostrils, may have had a good effect in arresting the first stage of epilepsy; and it may be worth while to try the effect of a bag of Camphor suspended round the neck, such as it was the custom for persons exposed to cholera contagion to wear.*

If, however, our efforts to arrest the first stage fail—and, for my own part, I have never seen them succeed—then we must address ourselves to the task of discovering some medicines which so act upon the seat of the disease as to restore it to a normal condition—that is, *which have the power of reducing to their natural calibre the capillaries of the spinal cord and brain, and thus of removing that preternatural excitability on which it now seems pretty certain that epilepsy depends.*

On entering upon this the most important and most

* Mr. Kankivell, of Penzance, has found the administration of alcohol arrest a paroxysm. His paper on the subject will shortly appear in the *Annals of the British Homœopathic Society*.

difficult portion of our task, it is well that we should clearly perceive on what the difficulty depends. It arises in a great measure from the conflicting testimony in regard to the efficacy of particular substances—a difficulty always great in medicine, but unusually so in a disease like epilepsy, which is so uncertain in its course as to baffle the most careful efforts of the least unbiassed to arrive at positive evidence in regard to the utility of any given drug. When we read the works of recent writers, we are struck with the acumen they display in their critical demolition of the statements of their predecessors and contemporaries. For example, Dr. Radcliffe analyses Trousseau's cases cured by Belladonna, and reduces the number to 20 out of the 150 patients treated. From Dr. Radcliffe we expect some statement of his own success, which shall be unassailable by the process he brings to bear upon Trousseau. Dr. Radcliffe is an advocate for the employment of Naphtha, Musk, and Castor, and we eagerly look for the proofs of his confidence. What does he give us? "*I think,*" he says, "also I can point to at least a score of cases in which the fits have not only been lessened in severity by being deprived of their most ominous character—coma; but where the intervals between the fits have become so lengthened out as to afford good ground for supposing that the fatal habit may be altogether broken by a continuance of the same method." If Trousseau, instead of a detailed account of all the 150 cases which he had treated with Belladonna, had said *he thought* he could point to some scores of patients who had been benefited by

this remedy, how mercilessly would Dr. Radcliffe have commented upon the difference of Trousseau's thoughts or impressions, and the positive testimony demanded by Science before she can adopt the conclusion that Belladonna was the real instrument of cure!

I do not make these observations to discredit Dr. Radcliffe, for whose labours I entertain the highest respect, but to show how much easier it is to attack the positions of another, than to place one's own so as to secure them from being taken by a similar assault.

What we observe in regard to the therapeutics of epilepsy is one of two courses—either an empirical confidence in certain remedies, without a corresponding pathological collateral security, or conjectural measures derived from inferences drawn from pathological speculations, and as yet unsubstantiated by the successful adaptation of these hypotheses into actual practice.

Nor are these obscuring conditions peculiar to either the old or the new school of medicine. For example—on the occasion of a paper being read before the Medico-Chirurgical Society, Sir Charles Locock (the President) remarked, that in epilepsy, in which the paroxysm had a tendency to assume a periodic character connected with menstruation, he had been led to try the Bromide of Potassium, by an observation made by a German physician, that this medicine produced temporary impotence.

Sir Charles stated that he had treated fourteen or fifteen cases of epilepsy presenting this peculiarity with Bromide of Potassium, and that he had only

failed to give relief in one case; and that one of the cases so cured had lasted nine years.*

I call this an example of an empirical cure; for we cannot admit that, because a German physician observed impotency to follow the administration of Bromide of Potassium, that therefore this substance was specifically adapted for the treatment of what may be called catamenial epilepsy. However, as an empirical remedy, it may be worthy of our attention; and the fact that so accurate an observer as Sir C. Locock testifies to its utility, is certainly a strong recommendation to examine its claims by the light of our therapeutic law. As yet we have not such a proving of the Bromide of Potassium as to enable us to put it to this test. Other examples of purely empirical remedies resorted to largely by the practitioners of the old school of medicine (which affects such pharisaical contempt for the means it so frequently condescends to employ), are the following:—

Viscus Quercinus, or Mistletoe;† Cotyledon Umbilicus, and Indigo.

Of Viscus Quercinus, Dr. Fraser reports that, out of eleven cases of epilepsy, which he treated with this substance, nine were cured, one was moribund, and one died.

Indigo was employed in epilepsy first by Dr. Ideler, of Berlin, and subsequently by Dr. Rodrigues (*Revue Medicale*, April, 1855). It is rather remarkable that,

* *Medico-Chirurgical Transactions* for 1857.

† *On Epilepsy and the Use of Viscus Quercinus*. By Henry Fraser, M.D.

although Indigo is a remedy introduced into the Homœopathic therapeutics, it should not be included in the list of medicines which Dr. Laurie gives as suitable for the treatment of the various forms of the disease—although this list embraces no less than forty-six of our medicines. Certainly the chapter on epilepsy in Laurie's *Elements of the Homœopathic Practice of Physic*, must impress our colleagues of the old school with the enviable richness of our resources; and, at the same time, excite a wholesome respect in the minds of laymen for any human intelligence which knows how to use, as arms of precision, nearly half a hundred different weapons.

Cotyledon Umbilicus, like the two former substances, has its admirers and defenders. It, too, has been "proved" by one of our body. But the proving has not induced its reception as one of the accredited Homœopathic remedies.

I have now given illustrations of the empirical remedies recommended by high authorities in the old school, simply on the ground of their approved usefulness. If this were admitted, then we should not cavil at the absence of a satisfactory explanation of their mode of operations. But, unfortunately, there is not one of the specifics in vogue for the treatment of epilepsy, that has not a much larger number of deniers than of believers; and as the number of such specifics is so numerous that their bare enumeration would fill a respectable volume, it is clearly hopeless to attempt, by the simple empirical method of experiment, unguided by theory, to determine whether any

of them have really the virtues with which they have been accredited. Nor is there better chance of success if we pursue the opposite method; and if, having accepted the pathological doctrines *now* in fashion as the basis of our treatment, we administer medicines in obedience to them alone; for the whole history of medicine, if it shows anything, proves this—that every age supposed it had arrived at the long-coveted knowledge of the real and essential cause of disease. Now it is a poison; formerly it was a spasm; and against the spasm an anti-spasmodic was prescribed: and after the world had been, on the strength of this doctrine, swallowing anti-spasmodics for a quarter of a century, up rose some clever, bold man, who denied with such force, and argued with such cogency, against the notion of a spasm having anything to do with the matter, that the world voted itself in the wrong, and gave up taking any more anti-spasmodics. There seems a danger of our falling again into this error. The observations of anatomists and physiologists, in regard to epilepsy, are very important, and their speculations as to its cause very ingenious, and *possibly* true. But let us remember that there is no theory which has yet been universally received by all physiologists, and that the most approved, at present, has not stood the test of twenty years; while, on the other hand, the symptoms of epilepsy have been carefully noted for as many centuries; and if we acknowledge the sufficiency of our therapeutic maxim as a guide in other diseases, there is no reason why we should discard it here. Let us, then, not be led astray from the study of, the

symptoms of epilepsy, into the speculative region of the cause of these symptoms. At the same time let us carefully arrange these symptoms, so as to show at once both their natural sequence and their comparative importance, and then try whether we can exhibit any medicinal actions having a similar sequence, producing similar results; and, if so, whether the substances that do so ever cure epilepsy.

In limine, let us observe, that any medicine which is to effect a radical change in the condition of an epileptic nervous system, and not merely arrest the propagation of the exciting cause, must be one endowed with powers of long duration. For this reason I do not believe in the *cures* of epilepsy said to be effected by Musk and Castor; and science demands of Dr. Radcliffe something more exact and definite than his statement to the effect that he could point out, at least, a score of cases which had been practically cured by these fugitive remedies.

Let us now arrange the symptoms in the order of their occurrence, putting down only the invariable, which we may presume to be the essential.

1. Dilatation of the pupil of both eyes.

This takes place *before* there is any loss of consciousness, and is therefore not dependent upon general insensibility.

2. Paleness of the face.

3. Twitches of the muscles of the eyes and face.

4. Loss of consciousness.

5. Tonic contraction of the laryngeal and expiratory muscles.

6. Cry.
7. Tonic contractions of the muscles of the trunk and limbs.
8. Fall.
9. Dark-purple hue of the face.
10. Asphyxia.
11. Clonic convulsions everywhere.
12. Coma.
13. Sleep.

We have no right, on the Homœopathic principle, to expect any medicine to be effectual in the cure of epilepsy, unless its pathogenesis covers all these symptoms. But this is not enough—it must also be capable of inducing a permanent derangement of the functions of the brain and other parts of the nervous system, as indicated by some impairment of memory and apprehension, by a tendency to muscular feebleness, and by a general habit of slight spasmodic action, represented by the “starts,” faintness, momentary arrest of consciousness, &c., which constitute the most important inter-paroxysmal phenomena. We should also like to find, in our medicine, the power of producing somnambulism, which, as we have seen, has a strong resemblance with some well-marked epileptic conditions.

Let us now examine, by the proposed tests, our Homœopathic medicines. Belladonna fulfils the first condition we have laid down as essential. It is a medicine of long-enduring action. On this head Hahnemann observes—“In the smallest imaginable dose, when the symptoms of the disease make Bella-

donna the suitable remedy, it proves curative in the most acute cases; but, on the other hand, it is not less potent even in the most chronic cases; and in them the effect of one dose will endure for a period of three weeks, or even more." On this point I believe that there is no difference in the opinion of experienced practitioners of Homœopathy. Few medicines produce more enduring effects upon the animal economy than Belladonna.

1. Of its power to dilate the pupil nothing need be said. Belladonna is *the* mydriatic.

2 & 3. Paleness of the face, and twitches of the muscles of the eyes and face.

We have these symptoms accurately reproduced in Hahnemann's proving.

170. Distorted features.

171. Paleness of the face.

174. Sudden paleness of the face, lasting some time.

4 & 5. Loss of consciousness, and tonic contraction of the laryngeal and expiratory muscles.

In a case reported by Dr. Gray, of New York, we are told "that the patient's manner was apoplectic; respiration anxious, and attended with brazen stridulous sound." He afterwards speaks of it as a state of "partial coma." The narcotising power of Belladonna, and especially of its alkaloid atropine, is too well established to require further illustration. What makes Belladonna especially suitable for epilepsy, is the mixture of symptoms of stupor and spasms at a stage of the operation of the drug prior to the pro-

duction of the true coma. In this patient of Dr. Gray's, for example, the state of coma alternated with paroxysms of uncontrollable tendency to motion and rapid automatic movement. Here we have an exact *simile* of the first stage of epilepsy before the profound stupor sets in.

6. Cry.

Hahnemann has, among the symptoms he collected from Greding, recorded the following, 1322 :—" With a sudden cry, he trembles in the hands and feet."

The fall, the asphyxia, and the violent general convulsions, are by no means peculiar to Belladonna. All narcotics which have the power of producing a state of true coma, do so by causing, in some way or other, venous, instead of arterial, blood to circulate in the brain. After a certain point, the symptoms are not those of the drug, but those of venous intoxication ; and we must be on our guard not to argue, from the appearance of *post-coma* convulsions among the effects of any drug, that it therefore possesses any true *spasm-causing* power. The experiments of Sir Astley Cooper abundantly prove, that the interruption of the flow of arterial blood to the brain is quite sufficient to induce violent *epileptiform* paroxysms. But the condition of the animal so treated is entirely different from those in a truly epileptic condition—such as Dr. Brown-Séquard induces by injuries of the central parts of the nervous system.

This is not so with Belladonna ; for the more we investigate the effects of this drug, the more am I convinced we shall find in it the symptoms bearing a

close resemblance with all the essential ones of epilepsy; and if we pass from the observed phenomena to their probable causes, I believe that the reason why Belladonna produces the image of the natural disease, is because it has the power both to induce in the central parts of the nervous system a morbid congestion, and also to excite, in the peripheral nerves, a morbid super-sensitiveness to impressions; so that while, on the one hand, it predisposes to convulsions by accumulating blood in the spinal cord, medulla oblongata, and brain; on the other hand, it facilitates the operation of the exciting causes of those convulsions, by rendering the surfaces of the body (both the external—the skin, and the internal—the mucous membranes) more liable to be offended by every irritant.

The expectations raised by a study of the symptoms produced by this wonderful drug, when given as a poison, or to ascertain its action in the healthy person, have been fully realised by the effects observed of its administration in the treatment of epilepsy.

The curative efficacy of Belladonna in epilepsy is attested by so many trustworthy observers, that my surprise is that it has not won a more general acceptance by the medical profession at large. My own experience in favour of Belladonna is, that it actually cures this disease even when it presents itself in its most formidable character. I may give one or two cases in illustration of the grounds of my confidence in this medicine.

A. G.—Oct. 17th.—A well-grown and intelligent lad became subject to epilepsy three years ago. After the first fit there was an intermission for two months; then he had two fits; after that they recurred at intervals of every two, three, or four months. He consulted me on the 24th of August, 1855. He had six fits on the previous day, and one that morning. The fits begin with convulsions of the muscles of the face, which extend to the arms and legs. He is generally unconscious for twenty minutes. He was ordered two drops of the second dilution of Belladonna three times a day. He took this medicine till the 11th of January, when he had one fit during the night. The medicine was continued till the 14th of July, and there was no return of a fit. I believe he has kept quite free of them ever since—he certainly was so for some years. The fits were of the true epileptic character, and, in some respects, bad; for there was very deep coma, and the length of a paroxysm was rather above the average period. He took the medicine for eleven months; and, as the fits came on about the age of puberty, and were increasing in severity and numbers for three years, the chances of this favourable termination being spontaneous are certainly not so great as that they were cured by Belladonna.

CASE II.—A fine intelligent boy, of 14 years of age, came under my care upon the 17th of December, 1855. His parents stated that he had been subject to convulsions as an infant, and from that time he had suffered from strabismus. His present malady has

lasted for one year. He is affected with an unpleasant sensation coming over head and hands several times a day, and one or two regular epileptic fits every day. His general health is good. On the 31st of December he began to take the second dilution of Belladonna three times a day, and continued to do so till the end of April. During the whole of these four months he remained perfectly free from all epileptic symptoms or paroxysms ; and, so far as I know, he has been in perfect health ever since. The slight general uneasiness this boy complained of, is very characteristic of true epilepsy ; and I have no doubt this case was an example of that disease, and that it was radically cured by Belladonna.

CASE III.—A big lad, 19 years of age, of a very dull expression of countenance, and an almost idiotic gait and demeanour, was brought to me on the 16th of March, 1856. He had been suffering from epilepsy for six years. The fits occurred two or three times a day, but not every day. He was ordered to take a dose of the second dilution of Belladonna three times a day. He returned on the 2nd of April, and his mother reported that he had had two bad fits the last week. The medicine was continued. He remained free of all attacks till the 14th of May, on which day he had one fit. On the 23rd of July he was brought to me again. His mother assured me that there was a marked improvement in his general intelligence, and that the fits were less frequent and less severe. After this I lost sight of the case.

CASE IV.—A girl of 14 years of age, of healthy

appearance, was brought for my advice on the 20th of March, 1857. Four years ago—*i. e.*, when she was ten years old—she had her first epileptic fit. It occurred without any assignable cause, and was very severe. The fits returned at irregular intervals until a year ago, when they began to occur regularly every month. They last for about fifteen minutes, and end in sleep. When she applied to me she had been free for three weeks, and reckoned on one being due on the following week. The second dilution of Belladonna was prescribed, a dose to be taken three times a day. There was no fit from the 20th of March till the 22nd of June, and none between that and the 12th. of November. Thus, instead of eight fits in eight months, she had only two fits, and I believe she kept well from that time. As I find no mention made in my notes of the appearance of the catamenia, I presume that this change in the constitution had not occurred during the treatment, and that the amendment was due to the persistent use of Belladonna.

These four cases occurred when I was practising in Leamington, and, along with some other striking recoveries or improvements in similar cases, were so much talked of that I was consulted by a great many epileptic patients, and have ever since had a number of such cases under my care. I must make the mortifying confession that, although even in apparently very bad cases, I have been able to effect great improvement; yet that, in very many, I have found myself entirely baffled; and the tantalising feature of the affair is, that I find I cannot pronounce with any

confidence as to the probability of the issue in any given case. I know of no special indications for the use of Belladonna, nor do I know, beforehand, whether a case will get better or worse, or remain stationary. I have been disappointed in cases which looked the most promising; and, again, other cases which presented all the worst appearances have been the most benefited.

The next medicine to which I wish to direct your attention, is Hydrocyanic Acid.

In the number of the *British Journal of Homœopathy* for July, 1862, there is an elaborate article by Dr. Madden and Dr. Hughes, upon the action of Hydrocyanic Acid, and especially upon its relation to epilepsy. At the end of the article there is the following note:—"In the *next* number of the journal we propose to give a series of cases of epilepsy and other diseases treated by this drug. If any of our colleagues have had experience with it, or should be led to use it successfully in the direction indicated in the above paper, we should be greatly obliged if they would communicate to us their observations, that we may incorporate them in our series." Having been for long much interested in the subject of epilepsy, I have opened each of the successive numbers of the journal with great curiosity, to read the cases which are here referred to as having been treated before July, 1862, with this drug. Up to this time, however, they remain unpublished; and in the absence of all *à posteriori* evidence in favour of the power of Hydrocyanic Acid to cure epilepsy, we must examine even more

critically the proofs advanced in the paper just referred to.

Having already confessed how often I have been baffled in all my attempts to effect a cure of epilepsy, I need hardly say that I began the perusal of the article with a lively hope of finding the authors of it to be right in their belief that Hydrocyanic Acid deserved a place next to Belladonna in the treatment of this dreadful malady. This hope seemed to me the more legitimate, inasmuch as we had, from the same joint-pen, received so valuable an article upon the relation of Belladonna to this disease—an article which showed that its authors had carefully arranged in their proper order, and valued at their just proportion, the symptoms which characterise epilepsy. However, I must at once confess, that the more I studied the article, the less I was satisfied with the conclusions arrived at in it. I will state my opinions rather in the form of questions than of opposing propositions.

The first I would note is, whether there is any evidence of Hydrocyanic Acid producing a permanent impression upon the nervous system? To me it seems to act as a very intense and very evanescent direct sedative. Let us take, for example, a case recorded in the *Revue Medicale*, and quoted by Christison and Hempel, as well as by the authors of the article under consideration. It is quite a model case; and the substance of it is thus given by Drs. Hughes and Madden:—

“Very soon after swallowing a teaspoonful of the diluted acid, he felt a confusion in his head, and then

fell down insensible as suddenly as if struck by lightning." Let us pause to observe, that up to this point there had been no convulsions, nor any symptoms bearing any resemblance to epilepsy. And this is the rule, not the exception. Thus a case is related in *Hufeland's Journal*, and quoted by Wibmer and Christison, of a man who took a large dose of this poison, and, "after staggering a few steps, he sank without a groan (and without a struggle) to the ground. A physician who saw him on the instant found the pulse gone, and the breathing for some time imperceptible." To return to the former case, the narrative proceeds—"There was difficult breathing; small pulse, scarcely perceptible at the left wrist; bloating of the face and neck; *dilated* and insensible pupils; and lock-jaw. Afterwards he had several fits of tetanus, one of them extremely violent. In about two hours and a-half he began to recover his intellect, and rapidly became sensible." On this we have the following comment:—"The *epileptiform* loss of consciousness, the tetanic convulsions, and the spasmodic dyspnoea of Hydrocyanic Acid, are well marked in this case." I confess I cannot see the resemblance to epilepsy. It seems to me much more like a transient attack of apoplexy; for let us observe that the convulsions did not occur at all till after the bloating of the face, and the insensibility of the pupils, demonstrated that narcosis, or poisoning of the brain by venous blood, had taken place. It was a toxical repetition of Sir A. Cooper's experiments. The supply of arterial blood was suddenly cut off from the brain, medulla oblongata,

and spinal cord; and the consequences of this were insensibility and convulsions. And so little had the poison affected the nervous centres in a strictly morbid manner, that very soon he began to recover, and, so far as we know, was no more affected by this powerful drug than if he had been strangled and restored to life. In fact, the symptoms are those of strangulation; and, *as such*, they bear a close resemblance to *the effects* of an epileptic seizure, which, indeed, strangles its victim as effectually as if a bow-string were tightened round his throat. But again, I say, I see no proof of Hydrocyanic Acid acting directly on that part of the nervous system which is the seat of epilepsy. We miss entirely the *early* dilatation of the pupil before the establishment of unconsciousness, which is one of the pathognomonic symptoms of the malady.

Is there any evidence of Hydrocyanic Acid exerting a long-continued morbid influence? Are not its effects like those of Camphor—very powerful, but very evanescent?

Have we sufficient evidence of its action when given much diluted?—I mean, as we are in the habit of prescribing our medicines. Will the millionth of a drop produce any effect? We know that a pure stimulant is annihilated by dilution, as in the instance of Alcohol. May it not be the same with a pure sedative? These are very important questions to have answered before we place our confidence in this remedy. I trust, too, that before long we shall be favoured with a full report of the cases exhibiting the curative efficacy of Hydrocyanic Acid in epilepsy.

Cuprum, either in the form of the triturated metal, or of the acetate of copper, is in high favour in the Homœopathic treatment of epilepsy; and certainly it seems to fulfil the conditions we require of an anti-epileptic medicine; for its effects are both peripheral and central, and of long endurance. For example, Wibmer relates the case of a girl, of eighteen years of age, who was poisoned with a salt of copper, and in whom it produced convulsions, and then insensibility. Some days afterwards a certain amount of paralytic weakness of the arms remained; general disturbance of the whole system followed; and eventually she died. After detailing a number of cases of poisoning by the acetate of copper, Wibmer concludes with the following summary:—

“Various observations make it probable that copper acts upon the brain, and even more upon the spinal cord. We meet with headache; often irrational talk; slight deafness; but more frequently twitches, trismus, and almost tetanic stiffness of the limbs, as consequences of poisoning by this metal.”

In the proving of *Cuprum*, given to us by Hahnemann, we find numerous symptoms, and groups of symptoms, bearing a close resemblance to those of a paroxysm of epilepsy. Noack and Trinks observe, that *Cuprum* “is especially suitable for relaxed, irritable, and nervous constitutions, with weakness, and excessive sensitiveness of the nervous system.”

So far as my own observations go, I must admit that I have been disappointed in this remedy in the treatment of epilepsy; and I am inclined to think

that the action of copper is rather upon the ramifications of the nerves than upon their central origin. It is of great use in certain forms of spasms, arising from what might be called circumscribed reflex action, as in some kinds of colic, and choleraic spasms. It is likewise of great use in certain forms of oppression of the brain; but it does not seem to act so specifically on the upper part of the spinal cord, or medulla oblongata; it does not, like Belladonna, dilate the pupils. In short, although presenting many striking features of resemblance, in its effects, to the symptoms of epilepsy, it does not seem to hit the exact likeness; and is, perhaps, more suitable for various kinds of epileptiform convulsions, and for general choraic tremors, than for true epilepsy.

I have certainly seen decided benefit from *Arsenicum*—a medicine not nearly so much in vogue among Homœopathists for the cure of epilepsy. That *Arsenicum* does, however, sometimes produce a set of symptoms closely resembling those of epilepsy is undoubted, and well illustrated in the following case:—

“A girl swallowed a drachm of arsenic, and was in consequence attacked violently with the usual symptoms of irritation in the whole alimentary canal. After a succession of ordinary symptoms, a new train gradually appeared. Towards the close of the second day she was harassed with frightful dreams, starting from sleep, and a tendency to faint; with coldness along the spine, giddiness, and intolerance of light; and, on the fourth day, with aching of the extremities, and tingling of the whole skin. This symptom con-

tinued till the close of the sixth day, when she was suddenly seized with convulsions of the left side, foaming at the mouth, and total insensibility. The convulsions lasted two hours; the insensibility through the whole night. Next evening she had another and similar fit; a third, but slighter, occurred on the morning of the tenth; another next day at noon; and they continued to occur occasionally till the nineteenth day.”*

The gradual formation of the epileptic condition is curiously exhibited here. Over-sensitiveness of the peripheral nerves, so that there was more than the proper amount of stimulus borne inwards to the centre; while, at the same time, an abnormal state of the upper part of the spinal cord was engendered, in consequence of which there was preternatural tendency on its part to be excited. Here we had the train laid by one hand, and the match applied by the other.

It is further to be observed, in regard to Arsenicum, that it produces intermittent fever; and Dr. Brown-Séquard has pointed out, that the conditions of ague and epilepsy are reciprocally antagonistic. For these reasons, as well as on the grounds of having seen it do good, I am disposed to press this medicine on your attention.

Another medicine, which has seemed to me of unequivocal benefit, is *Naja*; and we should expect it to be so from the effects it produces when it enters the system in such a way as to exert, to its full extent, its

* Quoted by Dr. Black, in his article on Arsenic, in the Hahnemann *Materia Medica*.

morbific influence. In the reports of fatal cases from the bite of the *cobra di capello*, we have the most characteristic symptoms of epilepsy occurring in the proper order of succession. First, there are convulsive twitches of various muscles, showing a general agitation of the peripheral nerves; then come insensibility and violent convulsions, with foaming at the mouth. Dilatation of the pupil is also sometimes observed at an early stage. Thus, from Lachesis and Naja, we have every reason to expect good results in the treatment of epilepsy; and, so far as my experience goes, it confirms these expectations.

From Nitrate of Silver I have never succeeded in obtaining any satisfactory results. This may be owing to its popular use, and even abuse; for, as it rarely happens to us to be called on to treat a case of epilepsy which has not been previously under some course of medical treatment, in the majority of our cases this remedy has been given, and failed to do good. However, Dr. Gray, of New York, than whom we have no more trustworthy observer, tells us, that "epileptic attacks produced by moral causes—*e.g.*, impassioned lay preaching—are promptly and directly cured by a few small doses of this medicine."

Nux Vomica is certainly a most useful medicine in the treatment of epilepsy; but rather, as it appears to me, by removing the peripheral exciting causes, than by acting on the abnormal condition of the nervous centres. However, as I have seen undoubted benefit from its action in some obstinate and severe cases, we should not deprive ourselves of its use by any specula-

tive objections as to its inability to produce the cerebral oppression which characterises epilepsy.

The same observations hold good of Pulsatilla.

In thus winnowing, out of Dr. Laurie's fifty medicines, these five or six, I by no means wish to cast a slight upon the others; but, as these are clinical lectures, I have confined myself exclusively, or nearly so, to giving expression to the result of my own personal experience and observation. I only regret that it is not more satisfactory; and I trust that, by-and-bye, we shall have, instead of vague indications and general directions for the use of remedies in epilepsy, a collection of well-attested cures of this terrible affliction, so that we may discover, with some approach to accuracy, what are the peculiar indications on which we may rely for giving a preference to one remedy rather than to another, in selecting that which is the most appropriate to every particular case of the disease, towards the cure or mitigation of which our remedial efforts are directed.

LECTURE IX.

ON ASTHMA.

THE transition from epilepsy to asthma is natural, if we adopt a pathological arrangement; and even in the purely practical aspect of the two maladies, there are so many points of resemblance, that the study of the one makes the comprehension of the other an easier task. True, the contrast between epileptic and asthmatic persons is very great. Among the former, the majority are greatly below par in mental power and activity; while, among the latter, the reverse is the rule; and asthma has been described as, *par excellence*, the complaint of the intellectual class. It may be, that the rounded shoulders and stooping gait which indicated the man of literature—before literature took to the rifle—was mistaken for the peculiar formation of the chest, which is so characteristic of the asthmatic, that the accustomed eye can at once recognise the sufferer from this complaint the moment he turns his back; or, it may be, that the peculiar nervous constitution of the asthmatic is highly favourable to the development of intellectual vigour. So far as my observations have gone, they quite agree with the popular belief. The asthmatics I have known have been persons of unusual mental activity and energy.

So much for the contrast between epilepsy and asthma. The resemblance between these two diseases consists in their both being affections of the nervous system, of a profound kind, connected with its original

formation; which gives to it a tendency to be excited into a peculiar paroxysm, giving rise to certain sensations on the one hand, and, on the other, to violent spasms of certain muscles. In asthma, as in epilepsy, what may be called the head-quarters of the disease are in the upper part of the spinal cord, about the origin of the pneumogastric nerves; and thence it extends along the whole of their ramifications, giving to all the parts supplied by this great source of organic sensibility, a preternatural sensitiveness, and a tendency to spasm. The morbid centre of epilepsy is probably close to that of asthma; but the nerves thence proceeding, by producing a spasm of the throat instead of the lungs, give rise to the phenomena of strangulation. So contiguous, however, are the sources of the two morbid currents, that cases are on record of the usual epileptic fit being supplanted by an attack of asthma. One striking example of this transmutation is thus recorded by Dr. Salter:—

“The case was that of a man of about fifty years of age, subject to epilepsy. His fits had certain well-known premonitory symptoms, and occurred with tolerable regularity; I think about once a fortnight. On one occasion his medical attendant was sent for in haste, and found him suffering from violent asthma. The account given by his friends was, that at the usual time at which he had expected the fit, he had experienced the accustomed premonitory symptoms; but, instead of their being followed, as usual, by the convulsions, this violent dyspnoea had come on. Within a few hours the dyspnoea went off, and left him as well

as usual. At the expiration of the accustomed interval after this attack, the ordinary premonitory symptoms, and the usual epileptic fit, occurred. On several occasions (I do not know how many) this was repeated, the epileptic seizure being, as it were, supplanted by the asthmatic.”*

Let us take the analogy between epilepsy and asthma as our clue, and see whither it leads.

To begin with : what is revealed by the examination of the bodies of those who have died either of the effects of these diseases, or who, although dying of other maladies, have suffered from either of them ? As a rule, in neither has there yet been found any constant structural change which can be held as an organic cause of the symptoms ; but in both there have been, *occasionally*, such changes observed, and in both the morbid alterations have been in the nervous system.

That structural change is rare, we state on the high authority of Laenec, who says—“ Even at the period at which we live, when the eyes of medical men are particularly directed to the minute investigation of the anatomical character of diseases, I have met with many cases in which it was impossible, after the most minute research, to find any organic lesion whatever to which the asthma could be attributed.” So much for the rule ; now for the exceptions. In a case of fatal dyspnoea, the body of the patient who had thus died was carefully examined by the accomplished anatomist Beclard, who found, as the only possible explanation

* Salter, *On Asthma*, p. 44.

of death, a tumour on one of the phrenic nerves. Parry narrates a case of dyspnoea occurring in fits of aggravation, without any symptoms of local pulmonary disease; and, on dissection, he found morbid alterations in the upper cervical vertebræ, the result of syphilitic action upon the bones of the neck. Lastly, Dr. Gardener relates a case characterised during the life of the patient by paroxysms of dyspnoea; and the *post-mortem* examination disclosed, as the cause of the asthmatic attacks from which he suffered, a neuro-matous tumour of the par vagum.*

Sir John Forbes makes, upon this class of cases, the following practical observations:—"The influence of spinal irritation in producing palpitation and other irregular action of the heart is well known; and we apprehend, that many of the chronic dyspnoeas and irregular asthmatic affections which we meet with in persons who are deformed, arise as frequently from disturbance of the spinal marrow, produced by the distortion, as from the disease of the lungs themselves."†

In tracing the analogy, and observing the contrast between epilepsy and asthma, there is one point of difference which arrests the attention. It is, that while epilepsy is often fatal, asthma may be said never to be the *immediate* cause of death. There are, indeed, one or two instances on record, which perhaps may, by some, be considered as exceptions to this rule. Guersent relates two cases of infants who died of an

* *Ed. Med. Surg. Journ.*, 1850.

† *Cyclopædia of Med. Asthma.*

acute remittent dyspnoea, with quick pulse, præcordial anxiety, and dry cough, and in whose bodies no organic lesion whatever could be found.*

'Andral' relates the following case :—"A baker, of good constitution, twenty years of age, who had lived in Paris for only two months, and who had been affected for the last five or six weeks with a slight diarrhoea, presented, on the 10th of April, all the precursory symptoms of measles—redness of the eyes, coryza, hoarseness, and cough. The same state on the three following days. On the 14th the eruption appeared; the patient kept his bed. On the 15th the entire body was covered; entered La Charité on the evening of this day. The eruption was then confluent, and quite characteristic. Pulse hard and frequent; redness of tongue and lips; violent cough; no other bad symptom. Towards the middle of the night the patient felt some oppression; this increased rapidly; and on the following morning, the 16th, we found the patient in a state of semi-asphyxia; eyes full and prominent; face purple; breathing short and very frequent, performed both by the ribs and diaphragm; cough almost constant—some mucous sputa; the chest, when percussed, sounded well in every part; auscultation caused some mucous râle to be heard in different places. Of the eruption, there remained some pale spots just on the point of disappearing. The pulse preserved its frequency and hardness, and the tongue its redness. This group of symptoms seemed to indicate the existence of a pneumonia; however, the

* *Dict. de Méd. Prat.*, t. iii., p. 126.

pathognomonic signs of this disease were completely wanting. Could a simple bronchitis, by its extreme acuteness or sudden exasperation, give rise to so intense a dyspnoea? Could this inflammation, in combination with that of the *primæ viæ*, explain the very severe state into which the patient had so suddenly fallen? Be this as it may, the indications to be fulfilled were no longer doubtful. The internal inflammation must be diminished, and that of the skin recalled." Andral attempted to effect this after his method; and the result was, that under intense suffocation the patient died on the 20th. The *post-mortem* examination revealed no morbid change of the parenchyma of the lungs; only intense redness, and some croupy exudation of the lining membrane of the bronchial tubes. Andral, while, on the whole, inclining to consider the case as one of fatal bronchitis, seems somewhat staggered as to its pathology; for he observes—"Those who admit the existence of nervous dyspnoea and essential asthmas, might cite this case in support of their opinion; they would say that they had often seen the bronchial mucous membrane as intensely inflamed without any perceptible dyspnoea resulting from it; from this they would conclude that, in the present case, the dyspnoea was an essential disease, independent of the inflammation of the bronchi." I have quoted this case in full because of its pathological interest, both as raising the question as to whether this baker died of nervous dyspnoea, and also as a proof of the intimate relation between cutaneous and some pulmonary affections.

As the cases referred to, and the one quoted, are

almost the only ones recorded of presumed death by asthma, we may assume that in this instance the exceptions prove the rule, and that asthma is never fatal.

Why is asthma never fatal? If it be what it is generally represented as being, a spasm of the muscular structure which surrounds the extreme ramifications of the bronchial tubes just as they loose themselves in the air-cells, then the consequences would manifestly be, that the lung-cells would be deprived of the supply of air requisite to the maintenance of life, and we should have the symptoms of death by strangulation. For where is the difference to the individual, whether he be choked by a ligature round his windpipe, or—what is the same thing—by spasmodic closure of the glottis, so that no air is permitted to enter the tubes and be conveyed along them through their subdivisions to the lungs; or whether the air so entering be arrested at the extremities of these tubes, and the craving of the parched air-cells be left unsatisfied? In the former case the patient is suffocated by a wholesale process; and, in the latter, he is suffocated in detail; but in both cases he is equally suffocated, if by suffocation we understand depriving the lungs of the supply of air necessary for life. And what do we mean by suffocation? We mean that the blood passes through the lungs without being there renovated as it should be—regenerated we might even say; and that instead of living arterial blood, dead venous blood is poured into the left auricle; thence into the left ventricle, which, in its turn, discharges it upon the

brain, and produces there the effects which Sir A. Cooper describes as having been exhibited by the rabbits, whose carotid and cervical arteries he had tied—viz., coma, convulsions, and all the after phenomena of epilepsy. But in asthma we have, as nearly as possible, the converse of all this. So far from there being coma or unconsciousness, there is quickened perception; instead of convulsions, we have absolute stillness of every limb and muscle, with one grand exception—the muscles of respiration. All these, and none else, are in a state of violently exalted action. So that we may, without any conceit of language, call asthma *epilepsy of the respiratory apparatus*.

The respiratory apparatus consists of a certain arrangement of muscles and of nerves, by which the chest is expanded, and the lungs inflated. Clearly to understand the phenomena of asthma, we must fully comprehend these two sets of apparatus. The muscles of the trunk which are brought in aid of the common respiratory muscles, are thus described by Sir C. Bell:—"If we look upon the frame of the body for the purpose of determining what are the muscles best calculated to assist in the motions of the chest when there is an increased or excited action, we shall have little difficulty in distinguishing them; and we shall have as little hesitation in assigning a use to the nerves which supply those muscles exclusively. These muscles, in effect, we see powerfully influenced by deep inspiration, however excited. They are the mastoid muscle, the trapezius, the serratus magnus, and the diaphragm. They operate in a circle; and all would

be useless in the act of respiration were one to be wanting. The serratus magnus expands the ribs; but this it does only when the scapula, to which it is attached, is fixed; and unless the scapula be fixed, this muscle has no operation in breathing. The trapezius fixes the scapula by drawing it backwards and upwards. These two muscles must always correspond in action in order to expand the chest. Now let us see how the trapezius influences the operation of the sterno-cleido-mastoideus. The mastoid muscle elevates the sternum; but only when the head is fixed, which is done by the action of the trapezius on the back of the head and neck. To this train of connections we may join the diaphragm itself; since, without the action of the serratus, the margins of the thorax would sink in by the action of the diaphragm, and the force of that muscle would be consequently lost."

This description, by the masterly pen of Sir C. Bell, exhibits at once the peculiar form of the upper part of the back, and habitual attitude of the asthmatic. His shoulder-blades have been so often raised and fixed, to give purchase to his serrati muscles, that his shoulders have become permanently round.

So much for the external muscles of respiration. Let us now glance at the sketch, by the same hand, of the nerves devoted to associate these muscles in combined action. "The nerves on which the associated actions of *voluntary* and *excited* respiration depend, arise very nearly together. Their origins are not in a bundle or fasciculus, but in a line or series, and form a distinct column of the spinal marrow. Behind the

corpus olivare there is a portion of the medulla which belongs neither to the motor nor to the sensitive tracts, and which, on dissection, will be found to have more direct connection with the *corpus restiforme*. This *fasciculus*, or *virga*, may be traced down the spinal marrow between the sulci, which gives rise to the anterior and posterior roots of the spinal nerves." * * * "From this tract of the medullary matter on the side of the *medulla oblongata*, arise in succession, from above downwards, the *portio dura* of the seventh nerve, the *glosso-pharyngeus*; the nerve of the *par vagum*; the *nervus ad par vagum accessorius*; and, as I imagine, the phrenic and external respiratory nerves."

Thus the principal seat of the power which controls respiration is within a very narrow compass. Now, as epilepsy is held, by Schröder van der Kolk, to arise from a morbid condition of a small portion of the *medulla oblongata*, so probably does asthma, or, at least, the disposition to asthma, depend upon a chronic inflammation (shall we say?) of a contiguous part of this vital structure; and as the only conceivable means of a radical cure of epilepsy, is by remedies which act upon the origin of the disease, so I believe that we shall find that, to cure asthma, we must look not so much at the outward manifestation of the malady, as at its inward source. Suppose we are right in this interpretation of the pathology of asthma, and that the disease depends upon a morbid condition of what, for want of a better name, we may denominate, after Bell, the respiratory tract; what should we expect, to

result from such a condition of the origin of the nerves? Let us see what takes place in epilepsy. Here we have, in consequence of a similar state of the roots of the nerves of *common* sensation and motion, the following phenomena:—

1st. A peculiar morbid sensation—a modification of the ordinary sense of feeling, known by the term *aura epileptica*.

2nd. After this peculiar sensation has been borne inward to the centre, we have, by the law of reflex action, certain twitches in the muscles of the face and eye, because the roots of the nerves which supply these muscles have been excited by the in-borne impulse known as a sensation.

What have we in asthma?

Suppose the *nervus vagus* to be morbid at its root, what should we expect to be the phenomena? Why, that, as one of its offices is to endow the lungs, not with *common* sensibility, but with the peculiar sensibility, which enables them to perceive the presence of unoxygenated blood, and to transmit an order, so to speak, for a fresh supply of air, that they may be permeated by the vital fluid; so, when there is a morbid condition of the pulmonary branch of this nerve, there is, as a necessary consequence, a preternatural sensitiveness to the natural stimulus, and hence there is a cruel craving for fresh air, giving rise to a *sense* of suffocation. This sense of suffocation, when it arrives at the centre of consciousness and the origin of motion, immediately calls into the greatest activity the whole muscular apparatus at its disposal, to relieve the anguish pro-

duced by this intolerable sensation—this true mimic death—long drawn out—which the asthmatic endures: *a sense of suffocation*—but not suffocation—is the essence of asthma, for there is no lack of air in the lungs. Instead of there being too little air in them, they are distended almost to bursting; but the air does not satisfy the craving. The patient sits, with his mouth open, gasping like a fish out of water. From the mouth and nostrils, to the innermost cells of the lungs, there is no impediment to the entrance of the vital breath; but it does no good so far as his sensations go.

How does this affect the circulation? The over-distended air-cells do not permit a free entrance of blood through their capillary vessels: the consequence is, that along with an inordinate appetite for, not air, but vivication of the blood—this being what we mean when we speak of want of breath—there is a deficiency in the process by which the venous blood is renovated. Too small a quantity passes through: what does pass, however, undergoes the necessary changes; it reaches the left auricle in a sparing stream; but what gets there is arterial living blood, not dead venous blood. This limited supply of stimulating blood excites the heart, which propels it quickly, but feebly, through the system at large, and up to the brain. The pulse is rapid, and small in consequence; the skin shrivelled and cold; the brain often wonderfully clear, but unfit for any effort.

If the air, instead of entering, as I believe it does, the tissue of the lungs, were shut off, as our autho-

rites tell us it is, what would be the condition of the blood? Why, manifestly it would be venous; and we should have venous blood entering the left side of the heart, and thence transmitted through the brain. Now, if for hours this was to go on, how is it possible that the person could avoid manifesting the unequivocal symptoms of venous congestion of the brain? We know what these are—we know exactly how long it takes to induce coma and convulsions; and yet there is no instance on record of either coma or convulsions being induced by attacks of asthma, even although of the severest and most enduring kind.

Among the arguments usually advanced in support of what I look upon as the erroneous, though prevailing, doctrine of asthma being a spasm of the entrance of the air-cells, one, much dwelt on, is the instant relief which is given by what are considered sedatives. I believe there is here a double fallacy; first, that what is relieved is not a preventing spasm, but an uneasy sensation—*anxiety*—and that the so-called sedatives relieve this either in virtue of their specific action on the nerves, by curing the morbid condition of which this is a symptom—as happens when Ipecacuanha and Lobelia do it—or, when Chloroform and Opium are the means successfully employed, it is in virtue of their primary or stimulating, and not their sedative action, that they effect the change. Nothing exemplifies this better than the relief given by burning nitre-paper, one of the palliatives most frequently efficacious. So far from the

fumes of the nitric oxide, which is liberated by the combustion of the nitre, being of a sedative or soothing nature, they are intensely irritating to the air-passages; so that if a person who has *not* asthma attempt to breathe this vapour, he is certain to be attacked with a sense of suffocation and a fit of coughing.

While we regard the essence of asthma to be an exaltation of the peculiar or specific sensibility of the pulmonary branch of the *par vagum*, from some morbid condition of that portion of the spinal cord whence this nerve springs, as well as a morbid state of the other nerves of inspiration depending upon something vicious at their roots, so that they convey an unnatural stimulus to the muscles they supply, which induces them to take on a spasmodic action—just as those of the limbs do in epilepsy—we must, at the same time, remember that the functions of the *nervus vagus* are very complex, and that it is not merely a nerve endowing the lungs with their peculiar sensitiveness to the presence of venous blood in their capillaries, but also the source of nervous influence to the mucous and muscular tissues, which enter into the composition of the ramifications of the bronchial tube and air-cells.

“ Now let us inquire into the effect of the enfeeblement of this supply of nervous force to the mucous membrane and the muscles of the bronchial tubes, so beautifully delineated by Reisseisen. We know, by abundant physiological experiments, as well as by pathological conditions, that when any mucous

membrane is deprived of its natural amount of nervous influence, the consequence is, that the capillaries of the part so maltreated enlarge; and, if the process be of long duration, that, instead of secreting their natural mucus, they first become dry, and then secrete an altered or puriform discharge. This is well illustrated in the eye. Such a change takes place in the mucous membrane which lines the bronchial tubes and air-cells, under the influence of the insufficient supply of wholesome nerve-force they receive from the morbid *par vagum*. At first, along with dyspnoea, there is dryness of the membrane; after a little time, the dryness gives place to an hyperæmic condition of the parts; and this, the second stage of the quasi-inflammatory state, terminates, as all inflammation tends to do, by resolution, and a discharge of some secretion from the mucous surface—at first slight, and viscid, and gelatinous, and afterwards more or less copious—sometimes extremely so—and very watery. Here, then, we see how naturally true nervous asthma associates itself with bronchitis.

Such are the consequences of a depraved condition of the mucous membrane, incident upon an insufficient supply of nervous force. Now what effect has this arrest of wholesome nerve-force upon the muscular tissue? Probably it induces, in the muscular tissue which surrounds the extreme ramification of the bronchial tubes, a condition analogous to that of the voluntary muscles of an epileptic person—viz., a tendency to assume spasmodic contractions, combined with weakness of their structure. And the result?

Obviously to obstruct the passage, into the walls of which they so largely enter; and to prevent, sometimes, it may be, the entrance into, but, probably, more frequently the exit, of the air out of the terminal cells, at whose mouths they are placed. If their spasmodic action prevent the entrance of the air in sufficient quantity, then we have the symptoms of asthma assuming those of true suffocative catarrh—a dangerous, but, fortunately, a comparatively rare form of the disease. If, on the other hand, the spasm shuts the door against the exit of the air, the consequences are enlargement of the air-cells, or perhaps their rupture, and emphysema of the lungs; and, as there is increased difficulty in transmitting blood through the lungs when they are in this condition—of course, to overcome this obstacle, greater force is required by the heart; and hence, along with emphysema of the lungs, we have associated an enlarged heart. But the enlargement of the heart does not imply an addition to the strength of its walls; on the contrary, along with dilatation of its cavities, there is often combined a degeneration of its structure; and we thus reach another link of the chain by which the asthmatic is in danger of being let down to his grave.

Emphysema—that is, permanent dilatation of the air-cells of the lungs, arising either from their over-distention in consequence of the difficulty of exit of the air from an obstruction in the passages through which it escapes, or from a simple over-action of a portion of a lung to compensate for the under-action

of some contiguous part, in consequence of the latter being supplied with a deficient quantity of air, or of its being unable to appropriate what we may call the nourishment out of its supply, from paralysis of its nerves;—emphysema, implying, almost as a necessary consequence, cardiac disease, is one of the most important and dangerous complications of asthma. When the heart is much affected, the change in its form and size can be detected by its pulsations being felt more markedly at the *scrobiculus cordis*. The late Dr. Todd, in speaking of this symptom, observes* :—

“I look upon this sign as one of the most characteristic symptoms of asthma; and I consider its presence, in every case where I suspect asthma, as a clear confirmation of the correctness of those suspicions. In accordance with this view, one of my first steps in examining a patient whom I suspect to be asthmatic, is to apply my finger to the *scrobiculus cordis*. If I find no beating of the heart there, my conclusion is a contingent negative. But if I find it beating there, and not in its natural position under the nipple, my conclusion is a certain affirmative.” I quote Dr. Todd’s expressions, not that I agree with them; for I believe—indeed, I am certain—that a person may have most unequivocal asthma for many years without the heart being at all moved out of its natural place; but I quote them because they give a valuable hint, which it is well to follow in the examination of all cases of asthma.

* The following narrative of a case treated in this

hospital last summer, well illustrates the danger of this complication. It is as follows:—

William Smith, a plumber and painter by trade, applied to be admitted as an out-patient on the 1st of January, 1862. He stated, that for several years he had been subject to a cough, which lasted generally the whole winter. This year it commenced in November, and continued ever since. The cough occurred in paroxysms, chiefly in the morning and evening, and was attended with little or no expectoration. He had difficulty of breathing with it, and, of late, occasional sharp pain about the region of the sixth rib, on the right side. There was nothing in his appearance at this time to indicate any unusual risk, and the case seemed like one of the many hundreds of winter catarrhs which crowd the out-patients' waiting-room. At the period of his admission he was ordered Bryonia, and the pain in the side was reported as being gone when he returned. However, on the 22nd of January there was an increase in the cough; and, in addition, he complained of more wheezing, and an uneasy sensation, rather than a positive pain over the region of the sternum. On the 6th of February he returned, and stated that the pain or uneasiness was gone, but that there was no improvement in the cough or dyspnoea, and that he had observed some swelling of his legs. He was admitted as an in-patient on the 13th of February. On admission, the expression of his countenance was anxious, and the skin was suffused with a dark-purplish hue; the respiration was hurried, and difficult. There was free expectoration

of muco-purulent sputa. There was no pain in the chest, except such as resulted from the frequency and severity of his fits of coughing. The skin was hot; the pulse 120—full, rather sharp; the tongue was clean; appetite good; bowels regular. There was considerable œdema of both legs.

His treatment consisted in the administration of Aconite and Arsenicum, and his strength was ordered to be sustained by appropriate food; and, if required, he was to have wine.

All the symptoms became steadily and rapidly worse, and he died on the evening of the 19th, having been six days in the hospital.

The result of the *post-mortem* examination, made by Dr. Neatby, who was then house-surgeon, is thus recorded:—

On opening the chest, about half a pint of serous fluid was found in the right pleura, as well as some old adhesions. Both lungs were emphysematous. On cutting into the trachea and bronchi, the mucous membrane was found congested throughout, and covered by purulent exudation. The smaller bronchial tubes were quite filled with this fluid.

In the pericardium about an ounce of serous fluid was found. The heart was of *double* its natural size. The left cavities were those chiefly enlarged. The walls of the heart were thin, pale, and fatty. There was slight thickening of the mitral valve. The tricuspid was normal. The liver was found in a state of fatty degeneration. The spleen and kidneys were healthy, as also were the brain and its membranes.

What strikes us in considering this case is, not that the man died, but that he continued to go about his work till within a month of his death. And the important lesson we learn from it is, that we should be very cautious not to underrate the danger of an attack of bronchitis, when it occurs in a person who has long suffered from asthma, or any affection of the chest, which predisposed the heart to be enlarged, and where emphysema is present. For we are apt to suppose, that if any one has been long subject to a winter cough, that there is no great danger in it; and if such an one consults us for apparently an ordinary attack of his familiar enemy, we may very easily overlook the fact, that his previous illnesses, so far from inuring him to their consequences, have each somewhat driven in the mine which at length was destined to sap the walls of his citadel of life. Nor is it so easy to determine the state of the heart as we are led to suppose from our books. When we put our ear to the chest of a patient who has long suffered from bronchitic asthma, we hear nothing but a confused sound of cooing, and whistling, and hissing; so that it is wholly impossible to ascertain whether the heart's action is natural in its intensity and rhythm. As to its position, it may be considerably displaced by enlargement; without our being able to detect it. For, be it remembered, that the whole cavity of the thorax is of unnatural size, and that the emphysematous lungs may quite overlap the apex of the heart, and prevent our discerning its situation by percussion; while the expansion of the parietes of the thorax, and their

unnatural position, prevent our accepting them as the landmarks for determining the point of impact. Although the heart-beat is felt most distinctly under the fifth rib, it by no means follows that the heart is not enlarged.

I believe, indeed, that we gather as much, or even more, from the general symptoms, than we do from the most adroit topical exploration of the chest.

The symptoms which indicated danger in the case just narrated, were—

1. The expression and colour of the face. In forming an opinion on a case of dyspnoea, we are much guided by the appearance of the patient. There is a well-marked difference between simple anxiety, however intense, and suffocation. The difference consists more in the colour of the skin than in the expression of the features. In some asthmatic dyspnoeas the face is always pale; in suffocative dyspnoea it is always more or less livid; and whenever we perceive this purple colour of the face, and especially of the lips, we may apprehend more or less of danger. There is also a great difference in the expression of the eye. In a paroxysm of asthma, however wearied this organ appears to be from want of sleep, it never assumes the dull torpid appearance which characterises orthopnoea, and suggests the danger of death being at hand.

2. Then we had here swelling of the legs. This is so notoriously a symptom of approaching danger to life, that there is no need of pointing it out as such: rather I would suggest the propriety of qualifying the unfavourable prognosis derived from this symptom in

certain exceptional cases. Œdema of the legs I have met with in cases of chronic bronchitic asthma, when they assumed an acute character; and I have known high medical authorities pronounce a very unfavourable judgment on the case in consequence; and yet I have seen the patients, on whom sentence of death has been passed by competent judges, entirely recover. In such cases the asthma has been complicated with gout, and the cedema has been due, in some way or other (which I do not venture to explain), to the gouty action exerted in the feet. The only means I know of by which we can discriminate between the cedema, which is the precursor of a fatal termination, and that which preludes a favourable issue, is this:—In the former, along with the progress of the cedema, there is a steady, and generally a rapid, aggravation of all the unfavourable symptoms; while in the latter there is a mitigation of these, coincident with the increase of the swelling of the lower extremities. Also we notice, that, in gouty cedema, one foot and leg is generally more affected than the other; while in the cedema from organic disease in heart or liver, both lower extremities are generally pretty equally affected.

3. The pulse was 120, full, and rather sharp. There was nothing in this rate or quality of the pulse in itself suggestive of danger. What gave a bad significance to it was, that it was not affected by Aconite. I always augur unfavourably of an acute disease, when the pulse either continues to rise, or does not fall under the steady administration of this medicine.

It may be useful to contrast, with this fatal case, one

or two others which terminated favourably. Of such we have had very numerous examples.

Edward Bristow, a porter by occupation, of 26 years of age, was admitted on the 10th of September, 1861. He states that, from his childhood, he has been subject to attacks of dyspnœa. That four years ago he had hæmoptysis, attended with cough, much expectoration, loss of flesh, and debility.

His present attack began some weeks ago, and has been steadily increasing ever since.

On admission his countenance denoted great anxiety (but there was no lividity mentioned in the report; and we may presume this symptom was not present). There was very urgent dyspnœa. He could not lie down on his back. The cough was very frequent, and followed by abundant expectoration of glairy mucus. Percussion-sounds clear. Loud mucous râles were heard, on auscultation, all over the chest. The skin was hot, and the pulse 120.

He was ordered Aconite, 3rd dil., gtt. ii. every four hours—to be followed by Bryonia, 3rd dil., gtt. ii. The report, on 12th, is—Very much better. Has slept well. Anxiety of countenance quite gone. Cough and expectoration both much diminished. Continue the Bryonia.

13th.—Had an attack of dyspnœa this evening, with loud wheezing.

R. Arsenicum, 3rd dil., gtt. ii. every four hours.

16th.—Much improved every way. Continue the Arsenicum.

21st.—Convalescent. He was discharged on the 22nd.

A recovery from so sharp an attack of bronchitic asthma in ten days, with a steady amelioration of all the symptoms after twenty-four hours' treatment, may surely be considered as indicating the curative power of the remedial measures employed.

There is one of the symptoms mentioned in the history of the case, on which it may be worth while to offer a few observations. I mean the previous hæmoptysis. It will be observed that this man had, four years before the attack for which he was admitted into this hospital, all the symptoms usually indicative of *phthisis pulmonum*—viz., cough, hæmoptysis, much expectoration, loss of flesh, and weakness. Now, I have met with cases in private practice, in which, from the presence of these symptoms, the patient was pronounced to be in a consumption, and which afterwards turned out to be cases of asthma, and not of phthisis. The differential diagnosis of these two very different, and, in fact, I believe, reciprocally exclusive conditions, is by no means always so easy as books would make it appear. It is often necessary to watch a case for some time before we can positively pronounce upon its nature. As a rule, the hæmoptysis of phthisis is different from that of asthma. The former, when not a copious hæmorrhage from the lungs (and such an event is an exception), consists of a slight admixture of blood with the expectoration—a mere *streak*, as it is generally called. In asthma, the blood is not mixed with

the expectoration; it comes up pure, and generally florid. It seems to give relief; and is probably a spontaneous hæmorrhage, by which the hyperæmia of the capillaries is relieved. It indicates no permanent structural change in the pulmonary tissue. In fact, it resembles the pulmonary hæmorrhage which we so frequently see in hysterical patients, especially when there is a suppression of the catamenia. I look upon hæmoptysis as a good symptom in asthma; but in phthisis it is always bad. The asthmatic generally improves rapidly after it, and is often for a long time free of a paroxysm of dyspnœa: it is not, in him, attended by hectic flushings in the face, or wasting night-sweats. Whereas, in the consumptive patient, hæmoptysis generally is the precursor of an increase of all his sufferings—it is the index of a fresh invasion of his deadly tubercles upon some fresh portion of his lungs. I will conclude this lecture with the narrative of another case, which I find entered in the Journal of the hospital as asthma:—

Jane Stafford, 24 years of age—unmarried—a domestic servant by occupation, was admitted on the 5th of December, 1861.

She stated that, on the 26th of November, she began to suffer from pain in all her limbs, and indisposition. She was so unwell as to be obliged to keep her bed. On the 28th she had an attack of shivering, which was followed by alternate heats and chills. On the following day, the 29th, the dyspnœa became much worse, and then she began to cough. The cough was slight, and attended with sharp pain under left mamma.

She suffered, besides, from a sense of burning below the sternum.

On admission, the respiration was hurried, and attended with much difficulty. The nostrils dilated at each inspiration. There was great wheezing. There was little cough. The cough was unattended with any expectoration, and gave no relief. On auscultation, sibilant and sonorous râles were heard all over the chest. The pulse was 76, weak. The tongue was thinly furred. There was much thirst. The bowels were confined. The urine natural.

R. Aconite, 3rd dil., gtt. j. every four hours.

Dec. 8th.—Complains of feeling sick; much difficulty of breathing, with wheezing; bowels confined.

R. Nux Vom., 3rd dil., three times a day.

9th.—Breathing less difficult; sibilant and sonorous râles not so loud. No nausea. Bowels have been relieved.

R. Arsenicum, 3rd dil., gtt. j. three times a day.

13th.—Has slight cough, with scanty mucous expectoration. Dyspnoea *occasionally* considerable. Still dry sounds heard in the chest; appetite better; tongue slightly furred.

Continue Arsen.

20th.—Much better; dyspnoea almost gone; respiratory murmur natural; only now and then dry wheezing-sounds are audible; hardly ever coughs.

24th.—Dismissed, quite well.

This I look upon as a case of true incipient asthma, rapidly arrested. Its history corresponds exactly with that given by old sufferers from this complaint. They tell us how very often, in November, they begin suddenly to wheeze, and feel great uneasiness in the chest, attended with slight cough, or none at all, and no expectoration; how this difficulty of breathing became worse at some particular hour, generally in the early morning, and generally established itself into a habit of regular asthmatic paroxysm, of longer or shorter duration; and how, after some weeks, there was improvement, and a return on exposure to cold or any depressing influence, until at length they found themselves fairly enlisted among asthmatics. That this would have been the course of this girl's case if she had been left to nature, as it is called—that is, if, according to the precepts of young physic, we had let ill alone—I firmly believe; and in my next lecture I will enter into a detail of the remedies which I have found most successful in asthma, and the indications for their use.

LECTURE X.

ASTHMA (CONTINUED).

I PROPOSE, in bringing before your notice the curative measures which have been found most efficacious in relieving and curing asthma, to consider, first, what we must try to do by the general regulation of the ordinary conditions of life, such as food and exercise; then, what we are to expect from medicines.

As a rule, asthmatics are lean. If stout, their fullness is rather from fat immediately under the skin, than from healthy development of all the muscular and other tissues. It is a most important indication, in the treatment of an asthmatic, to remove this leanness. That we may do so, we must inquire into its cause. This is not far to seek. Whatever other nerves are affected in asthma, or whether any, there can be no doubt of the pneumogastric. This, as its name implies, is the nerve both of the lungs and of the stomach. When we speak of the pneumogastric being deranged in its functions, we mean that the nervous influence it transmits is either in excess, or defective in kind or quantity; and this may, doubtless, arise not from any morbid condition of the trunk or branches of the nerve, but from something wrong at that portion of the medulla oblongata where it has its origin. Practically, however, we must consider it as a morbid condition of the nerve. In asthma, it seems that a defective supply of nerve-force is transmitted to the lungs and stomach. What are the effects? On the

lungs, the result of this imperfect innervation is impairment of their proper function—viz., the transmutation of venous into arterial blood. This we know to be a very complex operation, partly chemical—probably, so far as the inhalation of oxygen and the liberation of carbonic acid is concerned—but also partly vital, so far as certain changes in the character of the blood-globules are effected under the operation of vital laws. The chemical displacement of carbonic acid may take place in the lungs during an attack of asthma, so that the blood which permeates the brain and system at large need not be of the deadly narcotic character of epileptic blood; but the asthmatic blood may be innutritious, from the imperfect regeneration it undergoes. It may be insusceptible of proper assimilation by the tissues, especially by the muscles; so that, irrespective of any deficiency in the powers of digestion, the asthmatic may get thin. In short, the asthmatic is starved, not because the stomach does not do its duty, but because the lungs do not do their duty. I do not mean that the primary digestion is perfect in asthma—quite the contrary; but I mean that, even if it were, still the asthmatic would not get stout so long as he was asthmatic. Let us look at a person suffering from diabetes. In this malady the appetite is often insatiable, and the food is rapidly converted into chyme and chyle; but instead of the chyle being further metamorphosed into healthy blood, it passes through the lungs without undergoing the changes required to enable it to nourish the tissues; and hence they all starve. In like manner, although

less in degree, the asthmatic is not properly nourished even by well-digested food.

But it is quite the exception to find the digestion unimpaired in asthma. As a rule, the gastric as well as the pneumonic branch of the great organic nerve is affected. Digestion is imperfect, or, at least, feeble; the blood which reaches the lungs after a meal is almost certain to excite in them an abnormal amount of irritation. Hence we find abstinence, or extreme moderation, enforced by all medical authorities. "Hunger and thirst," says John Floyer, "are the best cure for the asthma, especially little and very small drink." This maxim, although sound on the whole, is somewhat too sweeping. That repletion is injurious, is obvious enough; but abstinence may be carried too far. I have met with cases in private practice, in which patients were undoubtedly injured by living on too meagre a diet. As a general rule, I believe that we should order small quantities of very nourishing food: for example, nothing suits better than game. As to stimulants, they must be taken or not taken, just as they are found to help or to hinder the rapid and complete digestion of the food.

We must remember that asthma is a *nervous* disease—that the essence of asthma is the defective force supplied by the pneumogastric nerve to the lungs, stomach, and heart; and we must recollect that this nerve is pre-eminently the bearer of the influences of the emotions to these organs. Hence it follows that asthma is much under the influence of what is pleasant or unpleasant to the person; and that he may

do a hundred things with ease when in exhilarating circumstances—things which would be almost death to him if he were in a state of mental depression. Laenec mentions a case of an old asthmatic who was invariably seized with a paroxysm if his lamp went out at night, even though he were asleep at the time. Doubtless he had so frequently been overcome by a sense of fear, from feeling himself helpless in the dark, that the extinction of the light had become associated with this emotion to such a degree as to be excited by the incident even when he was asleep. Now, from this inter-dependence of emotion and asthma, we draw this practical inference, amply confirmed by experience—that the asthmatic, when exhilarated by pleasant society, and surrounded by what is agreeable, may eat and drink not only with impunity, but even with great advantage, many articles of diet which, if these conditions were absent or reversed, would act upon him like so much poison. The lungs of the asthmatic are like a fire, which, if low, and not supplied with a thorough draught, is put out by a shovel-full of coals; but if, on the other hand, the flame is bright, then the coals, instead of extinguishing, excite combustion with greater activity.

The same rule holds good in regard to exercise. If this can be borne, it does great good. But here, again, we must look to the conditions; and the patient should be under the guidance of his monitorial sensations. So long as he feels well, he is well; but whenever the exercise, instead of enabling him to throw off the asthmatic tightness, increases it, then he is hurting

himself. "Of all remedies," writes a confirmed asthmatic, "there is none so complete and lasting as a day of severe walking exercise—five-and-twenty miles over hilly ground, and across heaths. The strain must never be great. I begin slowly—almost saunteringly; and only increase my pace when it is pleasanter to do so than not. Towards the end of the day I can usually climb a hundred feet of cliff as fast as I can plant my feet. * * * Habitually, I can never run or go fast up hill. In this matter of exercise, it is of paramount importance not to overstrain. If I am winded against a hill, and stop at the top till I breathe freely, I can get up the next hill more easily, and so on; but if, when the first hill was surmounted, I kept right on, I should get up the next hill worse, and so on."* This gentleman does not tell us how he managed his eating during his five-and-twenty miles' walk. I should guess that he ate sandwiches, or something of that kind, during his short halts, and that the exhilaration of his whole nervous system was so great that the stomach rapidly dissolved the food into chyme, which was soon further matured into chyle, absorbed into the blood, and, being presented to the lungs when glowing like a furnace, was immediately consumed by them, and re-embodied as invigorating arterial blood.

As to climate, we hear constantly of the capriciousness of asthma in respect to this. Of course, by the word "caprice," so used, we merely mean to say that we are ignorant of the exact conditions of the climate most suitable to every individual form of the com-

* Salter, p. 264.

plaint. The stories told about the odd places where some asthmatics find relief are endless—such, for example, as that of the gentleman who, after struggling for existence in the pure air of Hampstead, being detained one night in the neighbourhood of the Seven Dials, was so comfortable in his chest, that he took up his abode in that uninviting district, and enjoyed, for years, comparative immunity from his former paroxysms of dyspnœa.

The use to be derived from such recorded instances of amendment is not by blind imitation; but if we can discover why the air of the Seven Dials was better for the asthmatic than the air of Hampstead, we may be able to give our patients useful advice. Now, when we reflect upon the pathology of the complaint, we arrive at certain general conclusions as to what we must seek and what avoid. The air the asthmatic breathes, like the food he eats, should be moderate in quantity; it should not be very pure, as it is called, but rather diluted, so as not to stimulate too much the preternaturally sensitive filaments of the pneumogastric nerves, and induce a paroxysm. An asthmatic should avoid all atmospheric stimulants, just as an epileptic should everything which he found an exciting cause to the aura epileptica. For, although it be not felt, there is an “aura asthmatica”—that is, a morbid influence conveyed inwards by the par vagum to the origin of that nerve, where it touches the spring, and starts into convulsive action the whole of the respiratory apparatus. On whatever cause the quality we term “bracing” depends, whether it be the excess

of oxygen or ozone, or whatever it be, this quality is dangerous to an asthmatic if it stimulate his pulmonary nerves; and hence the reason why city air, which has been pretty well exhausted of this too vivifying principle, is advantageous to such an one. Besides, city air is warmer than the surrounding country air; and this also is an advantage, because cold applied to the lungs is apt to wake up the dormant tendency to bronchitis, which almost always, more or less, complicates asthma. For this reason, damp air, although it be not stimulating, is bad—bad, as inducing bronchitis, by depriving the lungs of their natural warmth; for nothing robs the body of its animal heat like a fog—that is, a cloud, or water suspended in air; bad, too, on account of its depressing the spirits, and acting unfavourably on the emotional apparatus. Nothing shuts up the “bowels of compassion” like an east wind. A prisoner would have a poor chance if tried by a jury sitting for some hours in an east wind. Among the things to be avoided are all damp and cold places, wherever cold fogs prevail.

On the other hand, we find that asthmatics do well in climates tolerably mild, but not relaxing—where they can be much in the open air without the danger of a chill. There is, I believe, no place in Europe where, on the whole, an asthmatic is so likely to recover as in London. To this rule there are, of course, exceptions. Still, as a general rule, it holds good. The advantages of London are very great; and among them we rank the power of enjoying a great deal of exhilarating passive exercise.

Among the general expedients for the relief of asthma, bathing has always been held in high estimation. I had occasion to notice, in the previous lecture, the intimate correspondence between the external covering of the body, the skin, and the internal lining membrane of the lungs; and I quoted a case of measles which proved rapidly fatal by the development of intense dyspnoea on the retrocession, or, at least, the disappearance of the eruption. Although many writers, before the time of Hahnemann, pointed out the importance of the repulsion of cutaneous eruptions as an origin of chronic diseases, none attributed so much weight to this cause as the founder of our school. On this point I am tempted, for several reasons, to quote an observation of the late Sir John Forbes:—"Zacutus Lucitanus," he says, "seems entitled to the credit of first attempting the cure of diseases by restoring, by means of re-infection, cutaneous diseases that had been suppressed. See Etmüller, who terms this 'a noble discovery—*egregium inventum*.'" There is nothing ridiculous about it, either in the eyes of Etmüller or Forbes, or, indeed, in the eyes of any but the illiterate, ignorant, and prejudiced—to whatever school they who deride what they do not understand may happen to belong. Baglivi, who belongs to a very different category, has given us the following aphorism:—"Ex scabie repulso si asthma fiat [evidently assuming that repelled itch is a cause of asthma], cum scabiosa dormiendum est, ut scabies revocetur." I notice this rather for the curiosity of the advice than to recommend its adoption.

One of the most important class of palliatives for asthma is the smoke of various substances. Those most now in use are Paris's cigarettes. These are composed of Strammonium, combined with Opium and Camphor, and are often effectual in alleviating the attacks, if smoked at a very early stage. More powerful than Paris's cigarettes are the cigars made of the *datura tatula*: these also are now in great demand. They may be had of Savory and Moore. Also the stems and leaves of the *Datura Strammonium*, smoked in a meerschaum, or common tobacco-pipe. At one time the smoking of *Datura* and tobacco was more in vogue than at present. About fifty years ago, being then newly introduced, Strammonium cured, or at least greatly relieved, its per-centage of asthma; and till it became known how small this per-centage was, the remedy enjoyed the hazardous reputation of an infallible specific. One can hardly wonder that it should be eagerly welcomed by asthmatics, for it came well recommended. We find such testimony as the following given by many entirely trustworthy persons:—

“My fits generally continued, with short interruptions, from thirty-six hours to three days and nights successively, during which time I have often, in the seeming agonies of death, given myself over, and even wished for that termination of my miseries.” After having tested the virtues of Strammonium, he writes—“In truth, the asthma is destroyed. I can drink beer; eat of everything; and, if my mind were as free from perplexity as my body is from asthma, I would again enjoy my existence. I would rather be without life

than without Strammonium." Another very intelligent asthmatic thus describes his experience:—"Smoking tobacco, or Strammonium, is sure to give relief if it produces expectoration; and it will generally do so if, the moment I awake, I begin to do so, and persevere for three or four hours. Smoking, I am able to say, after fifteen years' practice, and suffering as much as mortal can suffer, and not die, is the best remedy for asthma, if it can be relieved by expectoration. I have been in the hands of all the doctors of ———, and still I say—*smoke*."

Why not? If it give such certain relief, is there any harm in the practice? There is. The evils of smoking, especially for hours together, are, that it has a deleterious effect upon the brain and organs of digestion. It acts as a narcotic, and tends to produce confusion of mind; and, if persevered in for a length of time, must certainly do serious mischief of a twofold kind—mischief to the brain, as the organ of thought, and mischief to the brain, as the source of a perpetual stream of nervous influence to the lungs, heart, and other organs of organic life. In asthma, the great risk is not in the paroxysms, but in the effects of the paroxysms on the heart; and the use of any narcotic weakens that organ, and predisposes it to become morbidly affected. For this reason, although I would not forbid smoking to any patient who had found it of great service, I would always caution such an one against excess; and, if possible, substitute for this dangerous palliative, remedies either adapted to the radical cure of the complaint, or at least innocuous.

Chloroform, in some instances, is said to have a marvellous effect in arresting a paroxysm, as illustrated by the following narrative of a case treated by Dr. Salter, who says—"A poor woman was brought into King's College Hospital at the time that I was House Physician there, supposed to be dying by those who brought her in. She was quite unable to move, and could hardly speak; but it was easy to perceive, from the violent action of the respiratory muscles, and the loud wheezing that accompanied it, that the suffocation from which she suffered was of the asthmatic kind. I at once administered Chloroform. After a few whiffs the spasm began to yield; and before I had given her enough to make her insensible, it had quite subsided, and her breathing was free. In ten minutes after entering it she left the hospital well."

I quote this case partly because it is one well known to asthmatic patients, and is often alluded to by them as a proof of the value of Chloroform. But, after all, what are the *facts*? A woman is taken into a hospital during a paroxysm of asthma; she is immediately seen and attended to by a physician, who puts her through what, to her, is an entirely novel process—the inhalation of Chloroform. The paroxysm subsides. Does this prove that the cure was effected by the Chloroform? Certainly not. The case was one of purely nervous asthma; and such cases, we know, are susceptible of sudden cure by any strong emotion. For example, a gentleman subject to asthma gets caught, so to speak, in a farmhouse, and, with great difficulty, mounts his horse, intending to jog along at the quietest pace in

search of an emetic—*his* cure. The horse runs off with him. He becomes thoroughly frightened, and by the time he has pulled up, he, too, was well. I do not mean to maintain that Chloroform may not arrest a paroxysm, but I do maintain that Dr. Salter's case is no proof of its doing so. We are told nothing of the woman's history; whether she was subject to such attacks, and whether she was in the habit of being taken to a hospital to be cured. It is what may be called a sensation case, well fitted to interest and startle the unprofessional reader, but one which is of little use for scientific purposes.

So far as my experience goes, in this country, purely nervous asthma is quite the exception; bronchitic asthma is the rule. Now, I do not believe Chloroform would do any good in the latter; and in the former, many expedients less objectionable than chloroformic intoxication will answer the purpose as well, if not better.

Although nervous asthma is by no means so common as the mixed variety, yet we frequently meet with it in girls between fifteen and twenty years of age. It is, in fact, an accompaniment, even a symptom of chlorosis. That an insufficient supply of good blood should produce dyspnoea, is in entire accordance with our views of the cause of asthma; and if asthma consist in an intensified desire for the renovation of the blood at the lungs, this intensification giving rise to a sense of suffocation, we should expect to find it provoked equally by the withholding of blood as by the obstruction of the air-tubes. To use a figure of

speech, respiration might be called the marriage of air to blood: to effect the rite, both partners must be present; the absence of either is fatal to the nuptial contract. In chlorosis the blood is in fault; and we cure this form of asthma, on the whole, very readily by a course of Pulsatilla and Ferrum Aceticum. There is no medicine which is held in greater esteem by both schools of practice, for the relief of purely nervous asthma, than Ipecacuanha.

The following case is a good illustration of its mode of administration by our colleagues of the old school, and the usual explanation they give of its effects:—

“The patient was a youth who had been asthmatic from his infancy. His attacks had increased in frequency till, at the time to which I refer, they occurred with tolerable regularity once a week. His asthma generally awoke him about four or five o'clock in the morning, and soon compelled him to sit up and wheeze in bed, or to get out of bed, and stand against some piece of furniture for support. In two or three hours he would be able to dress himself; and, perhaps, in the forenoon the severity of the dyspnoea would a little abate; but towards the afternoon and evening it would deepen; and towards bed-time got so intense, that, without an emetic [*i.e.*, a dose of Ipecacuanha, for no other was ever used], he had no rest. The emetic [*i.e.*, the Ipecacuanha] would be taken, and, in half-an-hour, he would be perfectly easy, without the slightest trace of asthma. He would then take a light supper, go to bed, and sleep like an infant, and have no more

asthma till that day week. In this way he would have fifty attacks, or thereabouts, in the year, and cut them short at night with fifty emetics [*i.e.*, doses of Ipecacuanha]. If he did not take the emetics, he passed a miserable and sleepless night, and was still bad the next day. Indeed, there was no definite end to the attack without it. The dose was about twenty grains of the Ipecacuanha powder; and, although he repeated it so frequently, it neither lost its efficacy, nor did him any harm. It was clearly not as an emetic that it acted [this we grant], but as a depressant [this we do not grant]; for the relief took place before the vomiting." "A quarter of an hour after swallowing the draught, a sense of nausea would be felt. * * * Besides, the stomach was always perfectly empty; there was nothing of which this could be relieved."*

This may be looked upon as quite a model case of that form of asthma which Ipecacuanha relieves. It was spasmodic, not bronchitic; and regular in its periods of recurrence. That the relief obtained by the medicine was due to its action, can scarcely be doubted; and it shows how even large doses may act specifically. That Ipecacuanha did cure these attacks of asthma in this boy, in virtue of its power to produce a similar condition to the asthmatic paroxysm, is a proposition which I do not require to prove *here*. By us it is universally so considered, but not by us alone. It is also looked upon in the same light by some of the highest authorities of the old school.

No one has stated it more clearly than the late Sir John Forbes; and his opinion, in regard to the action of Ipecacuanha being due to its Homœopathicity, can hardly be ascribed to an overweening prejudice in favour of our school.

“Akenside,” writes Sir John Forbes, “was a great advocate for the employment of Ipecacuanha. His practice was to give one scruple in the paroxysm to induce vomiting [‘which,’ he says, ‘failed not to produce great and immediate relief’]; and, in the intervals, he gave five grains every morning, or ten grains every other morning, which was also, he says, ‘generally attended with vomiting, but sometimes only with nausea.’ This practice he recommended to be continued for a month or six weeks. He says, ‘the medicine proved equally beneficial, whether it produced vomiting, or merely nausea. *It is probable that it would have proved still more successful if it had produced neither.*’” “Ipecacuanha is certainly a remedy of considerable power in the asthmatic paroxysm; but this seems altogether independent of its emetic properties.” “Practitioners of experience,” continues Sir John Forbes, “without subscribing to the doctrines of Homœopathy, will certainly think more favourably of it—*i.e.*, of Ipecacuanha—on account of its peculiar tendency to induce fits of asthma in the predisposed. Long before the time of Hahnemann, the main principle of his doctrine was recognised by practical men in the adage, *Nil protest nisi laeditur idem.*” In short, according to Sir John Forbes, Ipecacuanha cures the paroxysm of asthma

because it is Homœopathic to it; and Sir John Forbes thus proclaimed himself a believer in the fundamental doctrines enunciated by Hahnemann.

So much for Ipecacuanha. The next medicine to which I would direct your attention is one not in the list either of Forbes or Salter, and apparently, therefore, not employed by the practitioners of the old school—I mean *Cuprum*.

I shall never forget a case which came under my observation some time ago, and which afforded as striking an illustration of the action of copper in arresting a paroxysm of asthma, as the case narrated by Dr. Salter, which he cured by Chloroform.

I was sent for, one May morning, about nine o'clock, to see a patient living at some little distance from my residence. I got to her temporary quarters about half-past nine, and I found a lady sitting in her dressing-gown, in an arm-chair, in one of the severest paroxysms of asthma imaginable. The face was pale and agonised; the breathing excessively laborious, and equally unsatisfactory; her chest heaved; all the external muscles that assist in its inflation were on the strain; she could not move, but sat fixed, with her arms resting on those of the chair. She could not utter a syllable; and her only mode of expressing recognition was by looking up as if imploring deliverance from her anguish. The pulse was small and quick; the skin cold, and neither moist nor very dry. On listening to the action of the lungs, the respiratory sounds were those of *dry asthma*—so-called *spasmodic*—but the air entered the air-cells freely. There was no

impediment either to its entrance or its exit; but it did no good; it did not relieve the fierce craving for vital breath portrayed in every lineament of her countenance. Her husband, who acted as interpreter to her dumb agony, said that she had been in this state for twelve hours; that she was subject to attacks like this; and that they lasted generally for two or three days. She had tried various plans for obtaining relief, but all had utterly failed. Could I do anything? I gave her at once one pilule of the 2nd dilution of the Acetate of Copper; and, in the course of five minutes, the countenance expressed relief; in a few minutes more she was able to speak, and then I heard her voice for the first time. She said, "I am better." I remained with her for about a quarter of an hour, and, before I went, she was comparatively easy. I left some more pilules of the Acetate of Copper with her, with directions to take one every half-hour at first, and then at longer intervals. Her husband undertook to call for me in the afternoon to report progress. He came about five o'clock, and said that his wife had steadily improved; in short, the paroxysm was gone, and nothing remained but its effects.

The action of the Cuprum was so immediate and decided, that the patient—a lady of great intelligence and energy, who had for fifteen years been subject to asthma—from that time cherished the remedy as her grand resource against a paroxysm. The following year she had what she considered just such an attack, and she took the Cuprum, which, in the interval, had

often saved her; but the spell was broken—it did no good. I was sent for about six o'clock one morning, and found her *much* in the same condition as when the copper had proved so useful—that is to say, there was the same intense anxiety, the same inability to move or speak; but there was more moisture in the chest; and the attack was the consequence of a chill, along with an alarm. It was not pure nervous asthma, but mixed with bronchitis. Hence the cause of the failure of her remedies. This attack took longer to subdue; it went on for some days; although it began to show symptoms of amendment in about twenty-four hours. I found *its* specific to be Tartar Emetic.

In regard to the special indications for the use of Cuprum, I should say that it is best adapted for those cases which, so to speak, are altogether evolved out of the nervous system; originating in some abnormal condition of its centre, and this predisposing cause excited into positive action by emotional or intellectual exertion.

The power of copper to give rise to general spasms is abundantly testified. It also exerts a powerful action on the lungs, as indicated by Symptom 239 of Hahnemann's provings—"Spasmodic attacks of tightness of chest (engbrustigkeit); the chest is, as it were, drawn together; the breathing is difficult, even to suffocation. On the remission of these spasms there follows spasmodic retching; after which, in the course of half-an-hour, the whole attack concluded." We could not desire a neater miniature of an attack of

asthma than is conveyed by these few lines. As no name is put after this symptom, it was probably obtained by Hahnemann from some one under his own personal observation.

Copper is said, by Wibmer, to have no specific action on the pulmonary tissue. "The lungs do not suffer specifically in poisoning by the Acetate of Copper; only frequently they are found partially injected with blood, and the bronchial tubes filled with mucus." The slight action of this metal upon the parenchyma of the lungs, is the reason of its not doing much good in what is called bronchial asthma. There is, however, no remedy on which we may reckon more confidently in pure nervous asthma—especially if the paroxysm has been provoked by a moral cause—than Cuprum. I prefer the Acetate to other preparations, as the most rapid and sure in its operation.

Next to Cuprum I would place Nux Vomica. Next both, in order of administration, and in its adaptation to relieve nervous asthma. After the paroxysm subsides, it leaves a condition of the digestive organs for which Nux Vomica is the great remedy. The tongue is coated with a thick, yellow fur; there is often slight nausea, flatulence, and constipation. Besides, the breathing is seldom quite right; generally there remains a sort of physical memory of the struggle. The patient feels that no liberties must be taken, either of diet or exercise. Out of this secondary state of bondage, nothing will liberate so effectually as Nux Vomica.

But I would not confine this great medicine to the

second stage alone ; it often cures the paroxysm also—as we should expect it to do from the proving given us by Hahnemann. One of the peculiarities in the action of Nux Vomica, which makes it specially suited to the treatment of asthma, is, that the symptoms it evokes tend to appear chiefly in the early morning. Now this is very frequently the case with asthmatics. They dread the early morning ; for, on first awaking, they are very apt to be attacked by a fit.

The symptoms of Nux Vomica, which bear a resemblance to asthma, occupy two pages and a-half of Hahnemann's *Materia Medica*. They are, of course, far too long to permit of my quoting them here. There are one or two, however, so characteristic, as to be worth noting ; *e.g.*—

693. Shortness of breath ; she cannot inhale a sufficient quantity of air, not even when she lies down. This is attended with a quick pulse.

794 is emphasised, and runs thus :—“An asthmatic, constrictive narrowing of the chest on waking and rising up.”

702. Tightness of the chest and anxiety constantly increase for hours together ; so that the breathing gets always shorter, and perspiration breaks out, from time to time, over the whole body.

We have also this important addition :—

740. Palpitation of the heart.

741. On lying down after dinner, palpitation of the heart.

I call this an important addition, because it points to the special relation of Nux Vomica to the cardiac

branches of the pneumogastric nerve; and when we find along with asthma—*nervous* asthma—palpitation of the heart, Nux Vomica may be given in preference to Cuprum or Ipecacuanha, or any other medicine.

A very striking instance of the action of Nux Vomica came under my observation some years ago. A gentleman called on me in great alarm, and stated that, for several successive nights, he had been awakened by a sense of suffocation, and violent palpitation of the heart. He was temperate in the extreme in *all* things. His digestion was unaffected. His apprehension was, that he should some night die of heart affection. Finding, on examination, no organic disease, and considering the complaint to be nervous palpitation, I gave him some pilules of Nux Vomica, one to be taken on going to bed; the result was immediate cure after the first dose: there was no return of his dyspnoea and palpitation. Some time afterwards he had a threatening, which again was at once relieved by Nux Vomica.

Both Cuprum and Nux Vomica, besides giving relief to the paroxysms of asthma, are well adapted to eradicate its cause, and may be persevered in during the intermissions of attacks for an indefinite period.

When the paroxysm is not purely nervous—that is, when, in addition to the *sense* of suffocation, there are superadded some symptoms of bronchitis—when the pulse, instead of being small, sharp, and quick, is full, and what might be called heavy, as well as quick, then Aconite I believe to be the best medicine. In this condition I have seen great relief from its inhala-

tion. Some drops of the tincture should be mixed in a quart of boiling water, and the patient may inhale the hot vapour. Given in this form, it acts more rapidly upon the seat of the irritation, and we avoid the necessity of the patient swallowing liquid. It is sometimes of importance to do so; for, not unfrequently, one of the most troublesome symptoms accompanying asthma, is the evolution of large quantities of what is vulgarly called wind in the stomach. The stomach is distended by it, and, *pro tanto*, the descent of the diaphragm is hindered, and the sufferings of the asthmatic paroxysms are increased by this impediment of inspiration. It would be a waste of time to adduce evidence of the suitability of Aconite to the congestive condition of the ramifications of the bronchial tubes; and what is true of Aconite, is likewise true of Bryonia. Many cases of bronchitic asthma may be cured by these two medicines alone.

Belladonna is one of the medicines we find in the manuals as of use in asthma. I have often tried it; but, I confess, I do not think it is *exactly* adapted to either the true asthmatic paroxysms, or the subsequent condition of the lungs and bronchial tubes. If we read the proving of Belladonna as given by Hahnemann, while we find great stress laid upon a convulsive cough, accompanied with dyspnœa, we find no hint given of asthma. The action of Belladonna seems to be much more upon the laryngeal nerves than upon the trunk of the *par vagum*. Hence its greater suitability to Millar's asthma, laryngismus stridulus, or spasmodic closure of the glottis, than to true

asthma. *Laryngismus stridulus* is more closely allied to epilepsy than asthma is, and hence the greater fitness of *Belladonna* in its treatment.

After the abatement of the paroxysm, the remedy that most frequently gives relief is *Arsenicum*. This powerful medicine has the advantages of acting both on the nervous and mucous tissues, so that it is equally adapted to arrest the sense of suffocation which arises from a morbid condition of the *par vagum*, and the subsequent engorgement and superabundant secretion of the mucous membrane lining the bronchial tubes, and entering into the composition of the parenchyma of the lungs.

Thus we find, among the symptoms recorded of *Arsenicum*, the following :—

“He suddenly loses his health in the evening, though cautious in entering his bed, and lying down quietly; he feels a sense of constriction of the trachea, and a fine wheezing becomes audible.” Nothing could more exactly represent the condition of an asthmatic than these few words. Dr. Black (whose opinion on practical points it always gives me pleasure to quote, knowing, as I do, how successful he is in his practice) observes, in a note to *Arsenic*—“There is no medicine which manifests so frequently and so closely the symptoms of asthma.” He adds, that it is especially adapted to the treatment of this disease when turgescence of the bronchial tubes is present. It may be well to give a case in illustration. I choose one out of a great many which have been successfully treated in this hospital. I prefer this chiefly from its brevity :—

Mary Johnson, 14 years of age, was admitted on the 24th of December, 1861.

She states that, for ten years, she has been suffering from more or less difficulty of breathing, attended with fits of coughing. She has a paroxysm every morning on waking; it ends by copious expectoration of frothy mucus. On auscultation, loud wheezing, and blowing, and ronchi are heard.

Prescription—Ipecacuanha, 3rd dil., ter die.

Jan. 3rd.—No change.

Arsen., 3rd dil., gtt. 1, ter die.

This medicine she took till the 23rd—that is, for twenty days; at the expiration of which period she was dismissed, greatly improved.

I think there is a disposition, on the part of our school, to undervalue Arsenic as a remedy in purely nervous complaints. So, although the bronchial engorgement in the above case brought it under Dr. Black's category, I would by no means wish to restrain the recommendation of Arsenic to this class alone. Indeed, I believe its special suitability for bronchial asthma, is because a nervous lesion always lies at the root of this disease. And this is the reason why, so far as my experience goes, we do not find so much benefit from another bronchial remedy—the Bichromate of Potash.

Neither in the admirable proving of this medicine by Dr. Drysdale, nor in the numerous cases he has collected to illustrate its therapeutic action, is there

evidence of its either inducing or relieving symptoms characteristic of true asthma; although it ranks amongst the most trustworthy and generally applicable remedies for bronchitis, both acute and chronic. One exception, indeed, does occur; and, for the sake of accuracy in a matter of so great importance as determining the class of cases for which a medicine is suitable, it may be right to point it out. Dr. Drysdale writes—“I may note also a case (a child aged four) where the whooping-cough was treated with the usual Homœopathic remedies, and there remained only the single symptom of wheezing at night: this was cured by the 6th dil. of the Bichromate of Potash” (p. 48).

Antimony, both in the form of Antimonium Crudum and of the Tartar Emetic, is of the greatest use in certain cases of asthma. I have known it give immediate relief after most other remedies had failed.

The poisonous action of Tartar Emetic has been carefully studied, both by Magendie and Lepelletier, who agree in ascribing to it a power of specifically inflaming the lungs. “Its effects,” writes Lepelletier, “on the respiratory organs, is to produce dyspnœa in dogs which were in perfect health before its administration: the lungs were found hepatised; lost their colour, and scarcely crepitated at all. One would imagine,” he continues, with delightful innocence, “that, admitting its action in man to be similar, far from being useful, its administration would be *particularly pernicious* in the treatment of pneumonia.” It is not for the pleasure of obtaining evidence in favour of Homœopathy from the lips of

so thoroughly hostile a witness, that I quote this passage; but to point out, that one of the first symptoms noticed in the dogs was dyspnoea. Now, it is true that this dyspnoea was the precursor of pneumonic infiltration into the lungs; still, it was *its precursor*, not *its consequence*; and I believe it arose from the same cause as that of asthma—viz., an excitement of the sensitive filaments of the *par vagum*; and that it is owing to this relation of Antimony to the nervous system, indicated also in its emetic power, that it owes its special adaptation to the treatment of asthma, especially if the paroxysms have been very frequent and severe, and attended with nausea and faintness.

Another medicine well worthy of consideration is *Conium*. Among the indications for its use given by Hahnemann, we find—"Dyspnoea on walking; tightness of the chest in the early morning, on waking; tightness of the chest early in the day." This is italicised (*Chronic Dis.*, p. 176). Among the symptoms he records, are some very characteristic of true asthma, especially one observed by Dr. Franz, numbered 536. "The breathing, particularly the act of inspiration, is very difficult, as if the chest did not expand enough." We find also—

566. Fits of suffocation, *as if* the throat were obstructed with mucus. Also,

567. Attacks of suffocation, *as if* something choked up the passage of the larynx.

As if—not from obstruction by mucus, but only a sense of suffocation, truly characteristic of asthma.

The following able summary of the action of *Conium*

I translate from Wibmer, slightly condensing it:—
“The principal effect of Conium is upon the brain. To the disturbance of the cerebral functions, are generally, but not always superadded, convulsions and stiffness of different parts of the body, as the chin, the limbs, and the head; general trembling, with weakness, and partial or total paralysis—all of which indicate its disturbance and destruction of the vitality of the spinal cord. We observe, besides great anxiety, respiration is effected with difficulty, and the chest is oppressed; at first, the respiratory effects are rapid, but they afterwards become slow, and altogether cease. The symptoms point to a poisonous effect by this substance upon the brain, the spinal cord, and the ganglionic nerves.”

Although, from the proving, and from the experience of others, I believe Conium to be a useful medicine in asthma, I have prescribed it so seldom as not to be able to give any clinical directions for the exact class of cases for which it is suitable. It is now freely used by our colleagues of the old school; but I should apprehend that, in the large doses they are in the habit of prescribing, there might be danger of its producing unpleasant cerebral symptoms, if persevered in for any length of time.

The last medicine of which I have to speak is, perhaps, the most important. I mean *Sulphur*.

The asthma symptoms, in the proving of Sulphur by Hahnemann, are very characteristic. It would require too much time to note them all; they occupy nearly a whole page. I will quote one or two.

1172. "She had hardly fallen asleep at night when she lost her breath; she felt as if she were suffocated; she sprung up with a cry, and could not recover her breath; towards morning, she was affected with palpitation of the heart, and weakening perspiration."

1173. "After walking twenty paces, her chest feels as if it were bound up; she required always to stand still in order to recover her breath."

The sudden attacks of dyspnoea at night, and the inability to walk, are both very asthmatic symptoms. In regard to the latter, it is rather odd that asthmatics can walk up a stair more easily backwards than forwards. I don't know the explanation of the circumstance.

The symptoms quoted are rather to exhibit the character of a large number of those excited by Sulphur, than because they indicate the precise form of asthma for which this remedy is most suitable. To form a correct opinion upon this important point, we must regard, not the pathological analogy of asthma with epilepsy, but its alliance with gout and psoriasis. It is very common to find paroxysms of asthma alternating both with fits of gout and attacks of lepra and psoriasis. I believe the administration of Sulphur to be indispensable in the successful treatment of such cases. The medicine may be given in the form of our usual Homœopathic preparations, or it may be in that of Sulphur-baths and Sulphur-springs. I have seen the most unequivocal benefit to the health from the use of sulphureous water, in cases where the lungs and heart were *oppressed with gout*—to use a strong ex-

pression, but one which will be understood by those who have had much to do with this disorder. In such cases, Sulphur does good by driving the peccant matter to the surface; and improvement is preceded by sharp pains in the extremities, or by an outburst of an eruption of psoriasis or lepra, or some such cutaneous affection. However unpleasant the process, the result is frequently that the sufferer not only enjoys, for a considerable time, immunity from his asthmatic sufferings, but feels a sense of renovation and rejuvenescence as if he had passed through the cauldron of Medæa, which, perhaps, was a mythical representation of some hot Sulphur-spring!

I have now enumerated all the medicines to which I intended to direct your attention; and yet, of many rather famous ones — *e.g.*, Lobelia Inflata — I have made no mention. My reason, or my excuse, for such omission is, that in these lectures I profess to give the result only of my own observations and reflections, not to enter upon a critical survey of the whole field of asthmatic therapeutics. It so happens that I have been disappointed in Lobelia, and so I can say nothing in its favour. My impression is, that the class of cases in which it does good is very limited, and not very well defined; and probably most of those who came under our care have already tried it, and in vain. In seeking to enlarge our stock of remedies against asthma, besides being led by their presumed physiological relation to the nerves, whose abnormal condition causes asthma, we should have an eye to their other pathological

alliances. I believe it will be found that the medicines which are most powerful in curing asthma, are also of great service in intermittent fever, and that there is a close affinity between ague and asthma. We have, however, an excellent stock of remedies for asthma; and, on the whole, our treatment of this affection is attended with marked and most satisfactory success.

LECTURE XI.

ON FEVER.*

IN the first three months of this year we treated about thirty cases of fever in this hospital. The number may be too small to furnish data for statistical deductions in reference to the success of our treatment, as compared with that pursued in other hospitals; but it is sufficiently large to have embraced examples of almost every variety of the disease, and most of its complications.

All the cases *admitted* came from one source—a Night Refuge, where we enjoy an unenviable popularity, from the belief, among its inmates, that they have a better chance of recovering here than elsewhere. Observe, I emphasised “admitted;” for, in addition to those, a considerable number of the cases consisted of the inmates of this hospital. Our house-surgeon, Dr. Carfrae, was laid up: his predecessor, Mr. Robinson, who kindly volunteered to occupy the post of danger thus left vacant; then our present house-surgeon, were also laid up. Besides these, four nurses took it, and several of the servants; and, last of all, although in a mild form, the matron of the hospital; but none of the other patients. This shows that the disease is infectious, and also that it is only moderately so. Small-pox, for example, is *much* more infectious. Of this fact we have had a painful illustration. On the 30th of January, a ser-

* This Lecture was delivered in May, 1844.

vant-girl, whom we shall call A., was sent here by Dr. Drury. There was nothing about her, when he saw her in the morning of that day, indicative of small-pox. I saw her on the following day (Sunday), and observed one or two slight papular elevations on the face. As a precautionary measure, she was removed to a small room quite apart from the other patients, and a nurse was engaged to attend on her alone. During the few hours she was in the ward, she had no intercourse with any of its inmates, as she was in a very depressed and silent mood. One of these, who occupied a bed at the opposite side of the ward, and who had, so far as is known, never approached within some yards of A., expressed great alarm when she heard that there was a suspicion of A.'s case being one of small-pox, and entreated to be removed to another ward. Her request was complied with. Notwithstanding, in twenty-four days, B. showed symptoms of small-pox, and had to be sent up to the small-pox ward. Nor did the mischief stop here; for, one day, after B.'s removal, C., and afterwards D. and E., all took the complaint. We have here a proof that small-pox is extremely infectious, even before the distinct appearance of the eruption—a fact generally discredited, and even denied by some standard writers on the disease. Before leaving the subject, I may observe, that, on the whole, the cases did very well; and that, so far as we could judge, the disfigurement of the face was much prevented by the application of a saturated solution of Gutta-percha in Chloroform, to which a small quantity of Bichloride of Mercury was added. This

mixture forms a pellicle, which at once excludes the air, and arrests the maturation and consequent pitting of the pocks.

To return to fever—it, too, is contagious; although not so contagious as small-pox: and we observe that the great danger is incurred by persons being long exposed to its influence; and this should be a hint not to allow nurses to be many days or nights exclusively with fever patients. It is a notorious fact, that all who are long confined in fever wards, are almost certain to take the disease. “*In no single instance*,” writes Dr. Gairdner of the Edinburgh epidemic of 1847, “known to me, did a nurse (in the Infirmary) who had not had fever previously, remain for six weeks attached to a fever ward without catching the disease.” This observation of Dr. Gairdner is confirmed by an overwhelming mass of evidence to the same effect; and we may assume it as incontestable, that no average woman or man (for the clinical clerks are in the same plight) can endure unharmed, for any length of time, exposure to the morbid influences of a fever ward. Would it not be right, then, not to detail nurses for this especial duty, but to employ them at it only two or three days at a time, or every second night or day? The objection to this is, the danger that a nurse who has been attending fever patients, and who then passes to the duties of other wards, may carry the infection thither; and that the disease, instead of being confined to its own department, may spread through all the hospital. So far as is known, this danger is not great. “There are no

instances on record," writes Dr. Murchison, in his masterly treatise upon Fever, "where a medical man has been the medium of transmission of typhus to his patient or to his family, as sometimes happens in the case of scarlet fever." "I have visited," writes Dr. Gregory, "more than a thousand patients in fever—many of them ten, twenty, or thirty times; yet I am certain I never brought the contagion into my own family." It is satisfactory for those whose duty requires that they expose themselves to infection, to know that they do not involve others in their danger. But when we consider that no year passes without a large sacrifice of the lives of nurses and medical students, in consequence of their intercourse with fever patients, surely we must come to the conclusion that there has been too great supineness in the matter. For not only are those who attend closely on bad fever cases, almost certain to catch the disease, but they are very likely to have it in its severest, and probably fatal, form. By far the worst cases we have had here have occurred among the nurses, especially the night nurses.

Of the two fatal cases, one was a night nurse. Have we a right to expose nurses and young medical men to this tremendous risk? Certainly not, if we can, by a little extra expenditure and good management, avoid the hazard. And why should there not be a rule, to the effect that no nurse should be in a fever ward beyond a certain number of hours at a time, and should not return to the post of danger until the lapse of a certain interval? The same regulation

should be made in regard to clinical clerks and house-surgeons; and if such an order implied an increase in the paid staff of our hospital, depend upon it that, even in a merely pecuniary point of view, there would be a saving of expense in the long run. Prevention is always, I believe, not only better, but cheaper than cure.

Through what channel does the contagion enter the system? In a pleasant little volume, by Dr. Chambers, well worthy of careful perusal, at p. 47, you will meet with the following passage:—"I am inclined to think the *usual* path by which the poison enters is the digestive canal. It is mixed with the saliva, and carried down to the stomach, where it possibly may accumulate and be multiplied in the gastric mucus. During severe epidemics, it has been observed, that those who smoke—that is, stop up their mouths with tobacco, and spit out the saliva instead of swallowing it—are less liable to be attacked. And at an early stage, even after the poison has begun to act upon the system, the fever may be arrested by emptying the stomach, and thus apparently preventing the whole dose being taken up."

Dr. Chambers then adduces, in illustration of his opinion, an instance in which a case of fever was cut short by an emetic. He calls the case "Continued Low Fever;" and I believe he does not recognise any general distinction between such cases and those with which we are now occupied. If this opinion be true, we could have no objection to the treatment founded on it. *Tolle causam* is as much a maxim of Hahnemann's as of Hippocrates; and, if the *cause* really be

removable by an emetic, then we should certainly administer this nauseous remedy. For my part, I admit neither the assumption nor the inference. I believe that, in the vast majority of cases, the poison enters, not by the stomach, but by the larynx; and that no good, and possibly much harm, might come from giving emetics.

It is impossible not to be struck with the similarity of the effects of the poison of typhus with that of plague and cholera; and in all of them there is, in my opinion, an overwhelming evidence in favour of the deadly stroke being given to the nervous system at the lungs, where it comes most nearly and extensively in contact with the malignant influence in the air. In the deadliest form of the disease, known by the terrible name of *typhus siderans*, or blasting typhus, which destroys its victims in a few hours, and committed such havoc in the garrisons of Saragossa, Torgau, Wilna, and Mayence, during the wars of the first Napoleon, and was occasionally met with in the Irish epidemic of 1847-'8, and among the French troops in the Crimea in 1856—in these cases the poison seems to have acted at once upon the heart, and destroyed life by paralysing that organ just like a dose of Prussic Acid. One cannot help being struck by the analogy, in cases of this extreme violence, with plague and cholera. "Oh my heart!" is the exclamation we used to hear in the worst forms of cholera, when patients sank without any attempt at reaction. So striking a symptom did this appear to Dr. Marcus, who observed the epidemic on a large

scale in Moscow, that he arrived at the conclusion that cholera was a disease of the heart; while, on the other hand, Russell, writing of the plague at Aleppo, says, that when the sufferers were asked where the pain lay, they either answered hastily they could not tell, or, with a fixed, wild look, exclaimed, "Kalbi! kalbi! (my heart! my heart!)"

The poison of typhus, like that of plague and cholera—to which it is probably much more nearly akin than is supposed—strikes first at the heart; and, during the whole course of a case of fever, this is the organ we should most closely watch. As a general rule, we can form a pretty fair estimate of the vigour of the heart's action by the pulse at the wrist; but, unfortunately, in typhus this yields fallacious indications—unfortunately, because, to be certain of the state of the heart, we must examine the chest with the stethoscope, and so be brought into somewhat unpleasant proximity to the source of infection. The point we must determine is the force of the beat. In most severe cases, the impulse diminishes steadily, from the fifth or sixth day, to the day of recovery or death; at the same time, the first, or systolic sound, becomes fainter, and often entirely inaudible. While the heart is thus feeble in its beat, there may be a compensating over-action in the arteries, either of the body generally, or of certain vessels. Dr. Lyons has recorded cases in which the carotid, temporal, or iliac arteries pulsated with violence; while others were not sensibly affected: so we must not trust entirely to the pulse, at the wrist in estimating the condition of the

heart. If, however, we observe that it becomes quicker and weaker when the patient sits up, we may assume that the action of the heart is weak in proportion to the difference in the condition of the pulse between the erect and recumbent posture.

The lesions observed in the hearts of those who die of fevers, explain and confirm the importance of the cardiac symptoms. The muscular tissue is found soft, and is easily torn. This softening is said to be independent of the duration of the disease, or the age of the deceased.* In many cases it is confined to the left side of the heart; hence the faint or absent systolic sound. The muscles of the heart are not only wasted, but, on microscopic examination, the fibrils were found to have undergone the true fatty degeneration. The heart, then, may be said to be liable to acute fatty degeneration in all severe cases of typhus or continued fevers; and this proclivity is attributed, by the most competent authorities, to the partial paralysis of those branches of the sympathetic nerve which are distributed throughout its structure, and whose efficiency is an essential condition for the healthy renewal of tissue, on which its unimpaired vitality depends. Thus we find ourselves always thrown back on the nervous system as the *ultima ratio* of morbid action; and we feel disposed to exclaim—“Give us only the key which controls the nerves, and the whole art of medicine will be at our command.” Whether we have, or shall ever have, such a key, is a question we must postpone for the present; but I

* Murchison, p. 236.

moot it now to fix your attention on the importance of taking into account the condition of the heart in typhus fever, as the symptoms of the disturbances of this organ are not so demonstrative as those of some other organs which we must now consider.

Before leaving the heart, I would observe, that I believe the foundation of many of its functional and organic diseases is laid by fevers of different kinds. I have frequently traced long-continued palpitation to malarious fevers of hot climates; and some instances have come under my notice of sudden death, some months after recovery, from severe small-pox, owing, probably, to acute fatty degeneration of the heart.

Although typhus fever undoubtedly affects the heart, yet it is the brain and nervous system which give the earliest and longest manifestation of its malignant influence. The following symptoms are the most constant, and should rule our choice of remedies:—Headache. This is generally the first complaint. The pain is usually in the forehead; it is seldom acute; generally heavy, dull, aching. The headache in typhus differs from that of what used to be called gastric fever. I mean, by this latter term, a non-infectious, continued fever, which has no regular course; in which there is no eruption, and which is not attended with diarrhoea, or any intestinal affection. I find this fever amalgamated with what systematic writers designate *pythogenic*: but the fever I mean has no rose-coloured spots, and does certainly not always arise from imperfect drainage. I therefore prefer the old-fashioned name of gastric, which involves no theory of

its cause. I look upon it as a great mistake, naming diseases from their supposed causes—*pythogenic*, for example. It is much safer to name them from some constant and striking symptoms; indeed, a purely arbitrary name, if once adopted, is the best of any. Everybody understands what is meant by gout; but how few know the meaning of the term. So, I believe, all practical men recognise a *certain* form of disease when gastric fever is spoken of—a fever which, having many points in common with typhus, differs from it in this, essentially, that the pain in the head is much more intense, and does not tend to pass into delirium.

Delirium may be of short or long duration; mild, or so violent as to assume the character of acute mania, as it did in the famous Black Assize of Oxford, where the patients are described as running, “like madmen, about the streets, markets, lanes, and other places.” It generally comes on about the end of the first week. It seems probable that the transition of headache into delirium, indicates that the blood has become tainted. I have observed, that so long as only headache was complained of, although that might have lasted several days, and was undoubtedly caused by *typhus* poison, yet that such cases were frequently cured *right off*, as the Americans would say; but I never saw a case that had passed into the stage of delirium, which did not go through the other characteristic phases of the disease.

That the blood is poisoned in typhus, does not admit of any doubt. The corpuscles lose their proper form, and do not arrange themselves in regular order,

like rolls of sovereigns, but fall separate, and marked with spots. It coagulates either not at all, and looks like thin tar, or in a very imperfect manner. It is supposed, with some plausibility, that the chemical cause of this state of the blood is due to the increase in the quantity of ammonia. At all events, if ordinary blood be mixed with ammonia, all the changes characteristic of typhus-blood make their appearance; and there is apparently an excess of ammonia in the blood of typhus. For if a glass rod or slide, moistened with hydrochloric acid, be held so that a typhus patient breathes on it, there is frequently found deposited on the glass, crystals of chloride of ammonia. Besides delirium, we very frequently meet with deafness, partly due, doubtless, to the general blunting of the sensibilities of the nerves by the poison, and partly to the wasting of the muscles of the internal ear. Deafness often outlasts all the nervous symptoms; and the perfect hearing is not restored until *all* the effects of the fever have ceased.

Nothing is more characteristic of typhus fever than the state of the mouth. At first the tongue is coated with a white or yellow fur, and this corresponds usually to the period of headache; by-and-bye the tongue becomes dark, and very dry, and covered, as well as the lips and teeth, with sordes. These sordes consist of dry, epithelial scales; and, taken in connection with the almost constant thirst of a fever patient, represent to us the absence of all secretion from the salivary glands. It is probable that there is little or no secretion from the pancreas either. At least, the

action of the liver, and other glands, seems suspended. Let us pause a moment to consider the effect of this absence of saliva and pancreatic fluid. The function of these secretions we know to be the transformation of starch into sugar. Sugar is the great source of fat in the system. Deprived of this peculiar aliment, we should immediately waste, and become lean, unless we took a compensatory allowance of fat, in the form of cream or oil. The rapid emaciation in typhus arises, in some measure, from the want of saliva; and we draw some important injunctions from this condition of the organs. One is—"Never give amylaceous food to a fever patient; for there is no power in his system to convert the starch—which, as starch, is absolutely indigestible—into sugar." Another is—"Since the patient cannot make his own fat, let it be introduced into the system ready-made, as in sweet milk and water. Thus given in small quantities, it may be taken up, and will lessen the risk of sinking from inanition, and shorten the convalescence.

While the inaction of the salivary glands and pancreas may be looked on as a negative evil, like starvation—far worse consequences result from the inability of the kidneys to remove from the body the dead tissues which corrupt in the blood. It is no figure of speech, but a scientific fact, that we die daily. From the hour of our birth we begin to die. In health, the dead parts are washed away through the kidneys. In typhus, a patient dies much more rapidly and extensively; the dead tissues dissolve in the blood, and exact more work from the kidneys. Thus we find the daily amount of

urea rise from 400 grains to 530 grains, or about one-fifth.* The kidneys are, in fever, the main waste-pipe of the body. If, from any impediment, they are unable to fulfil their office, and relieve the body of the perilous stuff that circulates through all its tissues, the direst consequences are the result. To this cause is now ascribed one of the most deadly complications we meet with in fever—viz., convulsions. Convulsions seldom appear till about the middle or end of the second week. At whatever period they occur, they have been considered, from the time of Hippocrates to the present day, as almost certainly fatal. Death usually takes place either *immediately* after the first fit, or within twenty-four hours. No appearance is ever found in the head to account for the convulsions. Dr. Christison states, that in *every case* of typhus in which there were convulsions, and which had been investigated with a view to discover their cause, there was organic disease of the kidney. One of our two fatal cases terminated by convulsions. Unfortunately, owing to the great stress in the hospital, in consequence of the disabled state of our most efficient house-surgeon, there was no *post-mortem* examination made in this case; nor is there so full a record of the symptoms and treatment as we usually possess of cases treated here. The following, however, is its general outline:—The patient, Eliza West, was an elderly woman, over fifty I should suppose—who had acted occasionally in the capacity of nurse in the hospital, when we were hard pressed. She had formerly

* Parker.

been addicted to habits of intemperance, and was altogether a very bad subject for any acute disease. There was nothing about the case to excite apprehension till the morning of the eighth day, when suddenly there seemed to be an increase of the delirium. Her countenance assumed a strange look; and, in the course of the day, violent general convulsions set in, and terminated in death the same evening. As I said, there was no *post-mortem* examination; but there is every probability that the fatal complication, in this case, was due to some old-standing organic disease of the kidneys. We may safely assume this to be the cause, especially as the convulsions appeared at so early a period of the fever—nearly a week before the usual day—which seems to indicate a more than ordinary proclivity to affection of the kidney. The case, I believe, even had the cause been discovered at the commencement of the attack—which was impossible—could not have been saved by any treatment. It was one of those marked *hopeless* from the first; and the only question was, at what particular day the fatal catastrophe was to occur. We learn, from such cases, to keep a careful watch over the state of the urine; for, although the presence of albumen in it is by no means certainly indicative of organic disease of the kidney, yet it affords a presumption that there may be such a condition present, and gives a more than usual seriousness to the prognosis of the case.

Another very serious, indeed generally fatal, complication of typhus, is swelling in the region of the parotid and sub-maxillary glands. The only other

case we lost, in the recent epidemic, was from this cause. The following are the particulars, extracted from the notes taken by Dr. Carfrae:—Emily Bostle was admitted on the 17th of January, from the Night Refuge, which has been the source of all our typhus cases. Her age is not mentioned; but it might be about thirty-five years. From the first her answers were very strange; her face was flushed; and she complained chiefly of hoarseness. She was ordered

Hepar Sulph., 6th, gtt. ii. 3th horis.

The report proceeds:—

Jan. 19th.—Looks very much excited and flushed; and when questioned how she is, says, “all right.”

Bellad., 1, gtt. ii. 3th horis.

20th.—Looks better; less flushed and excited; she seems to have difficulty of breathing, from some impediment in the larynx.

Bryon., 2nd, gtt. ii. 3th horis.

21st.—Has diarrhoea.

Merc. Sol., 3, ter die.

22nd.—Diarrhoea better.

Continue.

23rd.—Both sides of the face, especially the right side, enormously swelled, and both eyes closed, from the œdema of the face.

Bellad., 1, gtt. ii. 3th horis.

Jan. 24th.—Swelling diminished; puffing about eyes gone.

Continue.

25th.—Swelling much the same. Pulse 100; skin cool.

26th.—Swelling very much increased.

27th.—Died.

There is one very important symptom not recorded in this report—viz., hæmorrhage from the ear. This occurred on the 25th. There was no *post-mortem* examination obtained of this case. It is to be regretted that we have not a fuller detail of its progress; but there were great difficulties in making anything of this patient. From the first she had a wild, and irrational, and excited manner, which made it impossible to obtain any information from herself; and there was nobody to tell us anything about her. The inflammatory swellings certainly decreased, at first, under the use of Belladonna; and perhaps it was not of them she died, but of some affection of the throat or larynx. Of course, we could only guess at the condition of these parts; for, owing to the swelling of the cheeks, the mouth could not be opened so wide as to permit an inspection of the fauces; and she could not, or would not, answer questions. It is, I believe, Dr. Carfrae's opinion—and he had much more opportunity of observing the case than I had—that she died of suffocation; but whether from swelling of the

fauces or glottis, or the surrounding tissues, it is impossible to say. I do not think, had the case been given us to treat over again, we should have acted differently. Belladonna certainly seemed very well indicated, and at first produced a decided impression : probably the blood was too deeply poisoned to permit of its salutary action at a later stage.

There was but one other instance under my charge of this complication. The patient was younger, and the fever less severe. In this case, the action of the Biniiodide of Mercury, given in the dose of one grain of the first trituration every three hours, seemed at once to check the progress of the swelling.

The swelling of the face and throat is not, as it seems to be, an inflammatory enlargement of the glands of the part, but an affection of the areolar sub-cutaneous tissue. When suppuration occurs, however, the pus insinuates itself between the lobules of the gland, which, on being examined after death, has the appearance of having been dissected out ; while the glandular tissue is found loaded with oil. It sometimes happens that larger portions of the areolar tissue slough.

When typhus presents this complication, it approximates so closely to the plague, that the celebrated Egyptian physician, Clot Bey, on visiting the London Fever Hospital some years ago, declared that certain of the cases in which there were these swellings of the glands of the face, had they been met with in Egypt, would have been regarded as examples of the plague. And a writer, well acquainted with the plague (Fer-

riar), has recorded, that, "Although the symptoms of eruptions and buboes be distinguished by individual characters in the plague, yet they do not depart, in their general type, very far from the symptoms of malignant fevers; for the latter are very commonly attended by flat eruptions, which physicians call petechiæ; and glandular abscesses are not unfrequent in them."

It is time we should notice the eruption of typhus. I confess, it seems to me that rather too much stress has been laid upon the various fever eruptions. That there is a difference between the ill-defined, dark eruption of typhus and the rose-coloured eruption of another fever, is manifest; but I believe that nothing is so variable as the appearance of the eruptions in fever—variable in degree, at least, if not in kind. Just look at scarlet fever, which derives its name from the scarlet eruption of the skin. Yet even of this we find given, by systematic writers, this strange species—*Scarletina sine eruptione*; which, in the vernacular, would be, a *scarlet affection of the skin, without any change of colour in the skin*. Now, it is well known, that by far the most dangerous examples of scarlet fever are those of this anomalous character. Is it not so with this pythogenic or enteric fever? May we not have, in short, a continued fever, differing from typhus, and yet undistinguishable by any eruption? The subject is too large to be embraced in a few paragraphs of a lecture; but, with all respect for the authority of those who have so ably advocated a division of the continued fevers of this country, into

three great families—viz., typhus ; famine, or relapsing fever ; and pythogenic, or enteric fever—I confess that I do not think that the line of separation is so distinct, during the whole course, as the writers on the subject attempt to show.

The differential diagnosis between typhus and enteric fever is, that in typhus we see one kind of eruption, and in enteric another ; and that in enteric fever we have diarrhoea, and in typhus, constipation. But we may have a fever without any eruption, and with diarrhoea, arising from typhus poison ; and how are we to designate this ? So far it is undoubtedly enteric, that the intestines are the seat of morbid irritation ; and so far it is typhus, that the cause is a poison which excites a peculiar effect on the system. In fact, the impossibility of distinguishing typhus from enteric is admitted by Dr. Murchison himself, to whom we are indebted for the unfortunate appellation of *pythogenic*. At p. 524 of his elaborate treatise, he says—“The eruptions are the grand distinguishing marks between typhus and pythogenic fever. * * * When they are absent, it may be impossible to say to which fever a case belongs. * * * It must also be remembered, that the mere existence of diarrhoea does not distinguish pythogenic fever from typhus.” So we come to this—that here are two diseases pronounced by great authorities to be radically different, and yet that no human being can tell in what this difference consists, so far as the symptoms are concerned. Let us observe, too, that the management of the case, the prospects of the

patient, and the credit of the doctor, are all involved in making a correct diagnosis in an early stage; that an error may be attended with most serious consequences; for, if the case be typhus, it is dangerously infectious; and if enteric, it is not so. We have much to learn yet about fevers; and it seems to me there is at present a premature attempt at arbitrary divisions.

It is right, however, to place before you the features of contrast, as sketched by Dr. Murchison:—

PYTHOGENIC FEVER.

1. Pink or rose-coloured throughout.
2. Undergo no change until they fade or disappear. Never converted into petechiæ.
3. Circular.
4. Isolated, and few in number.
5. No sub-cutaneous mottling.
6. Elevated above the skin.
7. Disappear, on pressure, as long as they last.
8. Appear on the fourth or fifth day.
9. Appear in successive crops.
10. Each spot lasts only three or four days.
11. Never present on dead body.
12. A large number does not indicate danger.

TYPHUS.

1. May be dirty pink or red at first, but soon become reddish-brown.
2. Become gradually darker, and are often converted into petechiæ.
3. Of irregular form.
4. Numerous, and adhere in patches.
5. Mottling common, in addition to spots.
6. Not elevated above the skin.
7. Do not disappear on pressure, except at first.
8. Barely before the seventh day.
9. Never in successive crops.
10. Many of the spots may last till the end of the fever.
11. Often persist after death.
12. Direct ratio between the number and darkness of the spots, and the severity of the cases.

It may be possible to arrive at some test for typhus fever. Certainly such a test is not afforded by the eruption, which may be present or absent, slight or severe, without affecting the essential character of the disease. In many of the worst cases we have had under our charge the eruption was scarcely discernible. In short, it is an extremely variable quantity, depending upon many conditions—such as the heat of the weather; the state of the skin; its colour, texture, &c.; and, therefore, a bad diagnostic symptom. So, also, is diarrhoea. In some cases it seems, like perspiration, to be critical and beneficial; in other and more numerous cases it is a new manifestation of the poison, and, therefore, a bad and dangerous complication.

Of all complications, however, bronchitis is the most common. It was suggested by the great pathologist, Rokitansky, that the affection of the bronchial tubes, in this country, was of the same nature as that of the intestines in Vienna—viz., a peculiar morbid condition, arising from the specific action of the poison upon the thin lining membrane, giving rise to a deposit of a substance somewhat of the nature of very soft tubercle. This notion has not met with much favour in the eyes of those who have had the best opportunities of observing the disease; and the opinion which now generally prevails in this country is, that there is nothing specifically different in the bronchitis of fever; but that this inflammation, like all other morbid processes, is, more or less, affected by the changes which have occurred in the blood before the bronchial tubes were implicated. The bronchitis we have met with

here has seldom given us much trouble, but has readily yielded to the usual remedial measures. •

What is called typhoid pneumonia—*i. e.*, the pneumonia which we meet with in typhus—is not, strictly speaking, pneumonia at all, but *hypostatic consolidation*. Of this most important complication, Dr. Murchison gives the following account:—"It usually commences at an advanced stage of the disease (eleventh to fourteenth day), when the general powers are weakest; but it may come on as early as the seventh day. Owing to the paralysed state of the pneumogastric nerves interfering with the respiratory functions, and *the diminished power of the heart*, passive congestion takes place in the most dependent parts of the lungs; while, at the same time, serum is effused into the pulmonary tissue, and there is increased secretion from the lining membrane of the bronchi. Pulmonary hypostasis, in fact, is always accompanied by more or less bronchial catarrh; *and it is this combination which is the common cause of death in British typhus*. This morbid condition, often mistaken for pneumonia, may come on suddenly, and rapidly extend to a fatal termination, in patients who have passed through the greater part of the attack without any very unfavourable symptom. A patient in this state may have little or no cough or expectoration. Indeed, the absence of cough, betraying, as it does, the utter inability of the patient to rid the bronchi of the gradually increasing secretion, is an unfavourable indication.° When there is expectoration it is tenacious and frothy, and often mixed with streaks or small masses of florid blood.

The respirations are accelerated to 40, or even to 60, but are laboured and imperfect; the pulse is correspondingly quickened, weak, and irregular: but occasionally, towards the end, both pulse and respiration are abnormally slow; the face and extremities are livid; the surface is cold and often clammy; and the patient is in a state of stupor, passing into coma. On percussion of the chest, there is dulness, which at the first is confined to the most dependent part of either lung: as a rule, it is most marked, not at the basis, but about the middle, or a little lower. Gradually the dulness extends; sometimes it is more extensive over one lung than the other; but it is always most decided at the back part. On auscultation, mucous and subcrepitant râles are audible over the entire lungs—over the resonant parts as well as the dull; while, over the latter, the breathing is either feeble, absent, or bronchial.”

I have quoted this passage in full because of its importance, and because we have had no opportunity, in this hospital, of observing *this most ordinary mode of fatal termination of British typhus*. We have lost no uncomplicated case of fever. Of the only two patients who did not recover, the one died of convulsions; the other, of swelling of the throat. Was this due to the treatment, or was it that we happened to have only a mild form of typhus under our care? That the latter explanation is not the right one, seems proved from the following fact. As before stated, all our fever cases came from a Night Refuge. But all the fever cases of the Refuge did not come here; a consi-

derable number were sent to the Fever Hospital; and of these many died. This coming to the knowledge of the occupants of the Refuge, induced them to beg the directors of the institution not to send them any more to the Fever Hospital, but to the Homoeopathic Hospital. I should be very sorry to say anything here in disparagement of any other hospital. I look upon the large hospitals in this country with great respect, and I believe that they do an immense deal of good. Least of all would I wish, by any words of mine, to injure any fever hospitals. I believe nothing but a strong sense of duty will ever induce any body of men to devote themselves to the holy and dangerous office of arresting or mitigating the perennial plague of this kingdom. But if we are right in the belief that we do actually *cure* fever, we occupy a wholly different ground from the practitioners of the old system of medicine. One of its ablest and most experienced exponents, Dr. Stokes—a great teacher—a member of the Medical Council—a man of the highest weight on all subjects connected with medicine, and who has, along with his gifted and lamented colleague, Dr. Graves, devoted special attention to fever—makes this explicit statement:—"We cannot cure fever; no man ever cured fever: it will cure itself. If you keep the patient till the fourteenth, the eighteenth, or the twenty-first day, he will recover."*

Why cannot we cure fever? Is there anything in its essential nature so entirely different from curable

* Stokes' "Clinical Lectures on Fever."—*Medical Times*, vols. xxix., xxxi.: 1854.

fevers? "Certain forms of tropical remittent fever, known as typhoid, or malignant intermittents, and jungle fever," writes Dr. Murchison, "occasionally present symptoms having a close resemblance to those of typhus; such as—a small, soft pulse; dry, brown, retracted tongue; dorsal decubitus, and great prostration; low, muttering delirium; tremors, and subsultus; contracted pupils, and even petechiæ. Some years ago," continues Dr. Murchison, "I had an opportunity of seeing many such cases in Burmah; *and it is a fact that they have often been described as examples of true typhus in the tropics.* In distinguishing the two diseases, the circumstances in which each is wont to appear should be borne in mind. Typhus fever results from contagion, or over-crowding; and remittent fever results from malaria, and is non-infectious. Typhus is rare in those countries where remittent fevers, of the characters described, prevail; and in countries where the two diseases have prevailed together, as in the Crimea, typhus is more common in the winter and spring; remittent fever towards the end of summer and autumn. True remissions are not met with in typhus, and the exacerbations which occur are usually nocturnal; whereas those of remittent fever are for the most part diurnal. The great solid enlargement of the spleen, so often noticed in malarious fevers, is not characteristic of typhus; while the peculiar eruption of typhus is never met with in intermittent fevers. Lastly, Quinine, which is often a specific in malarious fevers, has no beneficial effect in typhus." Here we have presented, by the pen of a most com-

petent eye-witness, two forms of fever. What are the characteristic points of contrast ?

First.—Typhus is contagious and malignant ; remittent is not. Contagiousness is an accident of a disease, and does not enter into our notion of its essence. The same disease—*e.g.*, cholera—is certainly contagious at one time and place ; and, as certainly, it is not contagious at another time, and in other places.

Second.—Typhus results from over-crowding ; the other from malaria. Over-crowding is a vague expression : one might as well say that the other resulted from over-wooding. What is meant by the term is, I presume, that a malarious, or noxious, influence is generated by certain conditions incident upon the crowding together of human beings. We admit that the exciting cause of the two is different : the one is a poison emanating from the ground ; the other is generated by the bodies of living men. But the symptoms produced by these two poisons are so similar as to be hardly distinguishable the one from the other.

The distinguishing symptoms are—

The period of the exacerbations. (In typhus, these occur *usually* at night ; in malignant intermittent, in the day.)

There is a peculiar eruption in typhus, which is absent in the other ; and,

Lastly—The spleen is more affected in the intermittent than in typhus.

But no one will contend that typhus fever would not be typhus fever if the patient got worse during

the day, had no eruption, and had enlarged spleen, which clearly proves that there is nothing in the symptoms of the one essentially different from those of the other. All that remains to us, then, of essential difference is, that typhus is caused by a human poison, and malignant intermittent by a vegetable poison. Will any one seriously maintain that this affords sufficient grounds for affirming typhus fever to be in its nature incurable, while the other is curable? I hardly think so. We are apt to forget that there was a time, not so very long ago, when ague was as deadly as typhus is now. From the year 1629, to the year 1636—that is, during a period of seven years—there died of ague, in England and Wales, no fewer than *ten thousand four hundred and eighty-four persons!* James I., and Cromwell, and William III., all died of ague in London. Cromwell probably anticipated that ague was to end his career; for he has recorded how it destroyed all his kindred. “*Matrem pietissam, fratres, sorores, servos, ancillas, nutrices, conductitias, quoquot erant intra eosdem nobiscum parietes, ac fere omnes ejusdem ac vicinorum pagorum incolas, hoc veneno infectos et decubentes vidi.*”^{*} Surely to the physicians of that period ague must have appeared as impossible to cure as typhus does to the great body of physicians of our day.

Let us look into this matter a little closer, and ask—What has struck ague out of the number of fatal diseases? The common reply to this is thus given by Dr. Murchison:—“Among the greatest

^{*} Boudin, 1845, pp. 126, 127; Murchison, p. 2.

benefits that medicine has conferred on the human race, is the discovery of the causes of disease, and of the measures by which they may be prevented.

* * * At the present day, owing to the almost universal drainage and cultivation of the soil, agues have, save in a few isolated districts, almost vanished from this country." I cannot but consider this passage as rather unfortunate. Was the discovery of the cause of ague reserved for the sixteenth century? Was it not a fact, notorious during previous ages, that fever dwelt among the marshes, and that those who exposed themselves to its influence were sure to catch it, and likely enough never to lose it? Again—Was it the doctors that insisted upon draining for the sake of the public health? Certainly not. It happens, indeed, as a perplexing coincidence, that the great drainage of the Bedford levels was begun about the period when the mortality from ague began to decrease in England; but it was not to improve the health of the inhabitants, but to enrich the proprietors, that this great enterprise was undertaken. It was not that suffering humanity demanded it, by the voice of medicine—for the cry of fever-stricken wretches had ascended to heaven for centuries; but that money was more plentiful, and engineering skill, from Holland and Italy, could be more easily obtained. Medicine can take no credit to itself for the drainage of England. But is this really the cause of fever and ague being no longer the deadly disease it used to be? In some measure—yes; but only in a small measure. If any one will take the trouble to

look into the *History of Agriculture in England*, he will see, that although a few very great works were achieved during the seventeenth century, yet it was not till the middle of the eighteenth century that there was any such general drainage as could seriously tell upon the mortality of a disease which was, before that, not confined to any district, but was endemic throughout the greater part of the country. Now, we find that, during a period of seven years—that is, from 1653 to 1660—there *died* in England, of ague, 10,466 persons. How many died of this disease during the same period of time, after an interval of eighty years—*i.e.*, from 1733 to 1740? Why, only thirty-one. And I believe that there was very little, and very unimportant, draining of England till after the stormy political times which marked the early part of the eighteenth century.

If it was not by draining that ague was prevented, how was it deprived of its death-shafts? The answer to this is—the disappearance of ague, as a cause of mortality, exactly coincides with the introduction of Cinchona Bark into general use in this country; and that the more we sift the evidence, the more we shall be confirmed in the conviction, that ague was not expelled by hygienic measures, but cured by its specific remedy. Indeed, strangely enough, Dr. Murchison, after loudly ascribing the lessened mortality to medical draining, makes one of the diagnostic distinctions between typhus and malarious fevers, that the latter are sometimes cured by the specific action of Quinine. If the most deadly form of malaria is at

this day cured by Quinine, why may not the same kind of fever have been cured in England, 200 years ago, by the same remedy? He will not allow it, because it is the fashion of the day to deny all true curative power to medicine. "Nobody ever cured fever," is the utterance of our modern oracle; and to believe in the possibility and actuality of real cures, is, now-a-days, looked upon as a proof of imbecile credulity.

Between the disciples of the new school and of the old, there is this fundamental distinction—that while we seek to *cure* fevers, they look upon them very much as cases of child-birth—entirely natural processes, to be watched and nursed, and not meddled with, except there be something abnormal in their progress. The strength of the patient is to be sustained. "If you want an epitaph for my tomb," said Dr. Graves, "put over it—'He fed fevers.'" "Only keep patients alive long enough," responds Dr. Stokes. Thus it happens, that sustenance of life and strength being the great design of the treatment in the old school, the greatest attention has been paid to the best means of accomplishing this end; and we have now a code of practical directions in regard to the management of fever, founded upon a very large experience, by able physicians, altogether unfettered by any preconceived theories. It is not my purpose here to detail or discuss this code. With very few exceptions, I believe, we may adopt it bodily. Certainly, all that refers to simple aliment, and the general rules which are to guide us as to the use of stimulants in fevers, we can

hardly hope to improve. Perhaps we may even go a step further, and admit so much of the chemical treatment as to allow our fever cases a drink, composed of water, acidulated with Muriatic Acid. The use of this acid was proposed first by the chemists, who noticed the alkaline condition of the blood in fever. It now rests on the additional foundation of large and successful hospital experience. I by no means press its use in combination with Homœopathic remedies: all I would do, is to suggest the question, how far it may be combined with them without preventing their specific action? I believe that this is a very important point, like the administration of alkalies in rheumatic fever—a point on which it would be presumptuous for any one to dogmatise, and one which must be settled by a careful course of experiments.

In the meantime, however, we adopt all the measures on which the old school is unanimous. We give the same food and the same drink; we seek to give air as they do; and use cold water, and every appliance which they consider conducive to the restoration of the fever cases submitted to their care. Besides thus carrying out all these measures, we give remedies we believe to be directly curative; but we do not, by so doing, exclude any of the appliances trusted to by the old school. It is insisted on by our opponents, that our medicines can do no good, because the quantities we give are too small to affect the system at all. Then they can do no harm. This, at least, is certain; and, being certain, it seems strange that a poor-law union board in Ireland, in a town where the mortality from

fever was very great, should decline the generous offer made by one of our body; who undertook to treat, for six months, all the cases of fever, without putting the authorities to any expense. Knowing, as we do, the danger he would himself have incurred had the offer been accepted, we may, for his sake, be glad it was declined. But it seems to me that, in the face of the results obtained in this and all Homœopathic hospitals, and in the knowledge that, without leaving anything undone which the present practitioners do; but adding to this the administration of medicines certainly harmless, and possibly beneficial, the Board of Guardians of the sick poor have missed a golden opportunity of conferring a benefit both on humanity and science. There may, however—although we know of none—have been technical obstacles in the way of giving Homœopathy a fair trial in the Belfast Union. One reason alleged for refusing was, the want of success we have met with in the treatment of typhus fever in this hospital. I think we are in a position to give this an unqualified contradiction. I believe that any one who has watched the treatment, will be entirely satisfied that no cases have died except those almost or altogether invariably fatal on account of their complications—convulsions and enormous glandular swellings; and I believe that, considering the severity of the cases, the recoveries have been, on the whole, more rapid than usually occur, even in the best hospitals, under the old system. I repeat—those who have watched the cases, will be satisfied that what I state is correct. Unfortunately

we cannot establish this by statistics, because the numbers are too small.

The time, I trust, is not far distant when our method of practice will be tested on a scale commensurate with its importance; and it is for us, in the meantime, to open our wards to the inspection of all earnest inquirers; and to detail, as I propose to do in my next lecture, the remedies we have given; the indications which guided us in the selection; and the inferences suggested by our experience in the treatment of typhus fever.

LECTURE XII.

FEVER (CONTINUED).

WE shall derive the greatest amount of instruction afforded by our clinical experience, if we select one or two specimens of the various forms of fever which present themselves for treatment, and detail their course.

Alfred B——, aged five years, was admitted on the 27th of May, 1862. On the 22nd, five days previous to his admission, the child appeared very languid, and complained of being very cold. On the 25th he was sick, and brought up his food. On the 26th, symptoms of fever were very much more pronounced. There was delirium, and oppression of the chest. On admission, the following symptoms were noted:—Countenance anxious, flushed, and pale, by turns. Pulse 150, weak; bowels opened; urine high-coloured; tongue much furred, with elevated papillæ; no eruption of skin; has been raving all the morning.

Prescription—Acon., 3, gtt. ii. ter die.

May 30th.—Great improvement every way.

Continue medicine.

June 4th.—Quite well.

This is entered in the cure-book, “Febricula.” It is not included among the cases of continued fever. It is a very simple affection, and yet highly interesting to the student. In the first place, although spoken of somewhat slightly in books upon fevers,

I find no sufficient explanation given of its cause. Dr. Tweedie is of opinion that febricula is only a mild form of typhus, or relapsing fever. Others ascribe these fever symptoms to exposure to great heat or cold, surfeit of meat or drink, or great mental or bodily fatigue. Was this child's complaint typhus in a mild form? Certainly not. The points of distinction between febricula and typhus are, chiefly, the rapid rise of the pulse from the beginning in febricula; the early delirium; and the equally rapid decline of all fever symptoms, and direct transition from severe illness to perfect health. Here we have no change in the constitution of the blood; we have only its too rapid circulation. This one abnormal condition explains the whole of the phenomena. But when we attempt to go a step further, and enquire—Whence this? then we are altogether at sea for an answer. Not from typhus-poison—not from over-heat in the month of an English May, nor cold—not from intoxication, or mental anxiety, in a child of five years old—nor from surfeit of food; for the vomiting, which took place at the beginning of the attack, would prevent any such long-enduring mischief from mere over-eating. None of these causes are sufficient to account for this short, but, while it lasts, very severe fever; and it seems to me that we have, as yet, no sufficient explanation of this common disease. It is a fever chiefly of the young. The mean age of 845 cases admitted into the London Fever Hospital, was only 22 years—very different from typhus in this. It is of great importance that it should be distinguished from

typhus. If it be included under the general heading of "Continued Fever"—being, in this country, never fatal, and generally rapid in its course—it entirely vitiates the returns from hospitals in which this is done; very much reducing both the average mortality and duration of their fever cases.

Although this form of fever is not often very serious, and is never fatal in England, it is otherwise in hot climates. It is known there as ardent continued fever, and is very intense in its primary symptoms, and followed by sudden, and, frequently, fatal collapse. For this fever, all writers on Indian diseases recommend venesection, or leeches to the head. Would it were possible to induce medical practitioners in India to give a fair trial to the medicine which, in this country—and, I believe, in India, too—is always successful in checking every case of pure fever, when the blood is untainted—I mean Aconite—the only medicine given to this child, and the only medicine required in the treatment of this large class of cases.

Let us place by the side of this case of febricula, one of typhus fever in its mildest form.

Charlotte R——, æt. 25, was admitted on the 18th of Jan., 1864. She had complained of headache for ten days. Two days ago this became much worse, and she was, in consequence, sent here from the Refuge, where fever prevailed at the time.

On admission, she complained of severe headache, with great weakness. Pulse 116; skin hot, moist; no eruption; tongue coated with brown fur; great thirst; no appetite; bowels costive; sleepless.

Prescription—Bellad., 3, gtt. ii. tertiâ quaquâ horâ.

To have beef-tea, and four ounces of wine.

Jan. 20th.—Has no headache. Is altogether better.

Continue.

21st.—Continues to improve.

Continue.

23rd.—Has had a sleepless night. No other complaint.

30th.—Discharged, well.

On a superficial view, this case may not seem so very unlike the last; yet a closer inspection will reveal points of quite fundamental difference. In the first place, it came out of a fever-nest; then the headache lasted for ten days, without delirium; the tongue was coated with a brown fur; the bowels were costive; there was no sickness; but no appetite, and intense thirst; and, lastly, there was long-continued sleeplessness. The illness of this young woman was the result of contagion. There was no *symptom* distinctive of typhus; but there can be no doubt that it was a case of typhus. It must either have been typhus or enteric fever. There was no abdominal tenderness. Instead of diarrhoea there was constipation. There was no rose-coloured eruption.

Of cases such as this we had many examples among the nurses and servants of the hospital; and, for the most part, Belladonna was the only medicine prescribed; and, so far as we could judge, Belladonna had

a positive influence in preventing the development of more serious symptoms, if the remedy were given in time, and if the patient's constitution were not so weak as to be, at the very first, overpowered by typhus-poison. When this was the case, then other symptoms supervened, requiring other treatment. But for all cases of mild typhus, in tolerably vigorous persons, I should advise Belladonna.

Let us now examine a case in which the poison bit a little deeper, and which did not yield to Belladonna.

Emma H——, æt. 19, admitted on the 4th of Jan., 1864. She cannot give any *history* of her ailment. Complains of severe headache; pain in back, and in all the joints; foul tongue, with thick, white coating; great thirst; anorexia; sleeplessness; bowels costive. Complains of tenderness of abdomen—worse on pressure; skin hot and dry. There are no distinctly marked spots. Pulse 120. There is a troublesome cough.

Prescription—Bellad., 3 dec., gtt. ii., 3^{ua} qq. h.

Jan. 8th.—Cough very troublesome, with “catching” pain in side; tongue cleaner. Pulse 114.

Prescription—Bryon., 3 dec., gtt. ii., 3^{ua} qq. h.

11th.—Is better in every respect.

Continue medicine.

12th.—Continues to improve.

15th.—Well, but weak.

In this case the indications for Bryonia were very well marked, and the recovery was most satisfactory. It may be said that it was altogether a mild example of the disease: at the same time, it is impossible to be sure that, but for the treatment, it might have been much worse. We know, as a fact, that the most fatal form of typhus in this country is that in which the chest symptoms predominate; and that of this form we have not lost one single case. We also know that Bryonia, besides being well adapted to the general symptoms of certain common forms of typhus, has a specific action upon the bronchial tubes. Surely we are entitled to infer, from these established facts, that the recoveries which occur under the use of Bryonia, are due, in some measure, to the action of this powerful medicine. Dr. Anelli, of Presburg, thus describes a form of what he calls typhoid fever, in which Bryonia was specific:—"Typhoid fevers are frequent in Presburg, and have almost always a peculiar character. They generally commence by a very marked coldness, accompanied by a vertigo, which lasts during an hour. It is replaced by an intense heat, which continues two days, decreasing little by little; then comes delirium, insensibility, and the patient dies, insensible, at the end of a few weeks, in a state of profound stupor. Its most remarkable feature consists in the absence of abdominal symptoms. Under allopathic treatment, most of the cases have a fatal termination. The same result occurs when they are left to the resources of nature. If a cure is being effected, then, towards the end of the twentieth day, a complete deafness comes on.

This critical phenomenon of good augury begins on the fifth day by Homœopathic treatment, under the influence of which the mortality descends to zero. Bryonia is the medicine indicated, and sometimes suffices, of itself, to bring the disease to a good termination.*

The following case is an example of another and more severe form of the disease :—

Robert C——, aged 26, admitted on the 5th of Feb., 1862. Has been ill for a fortnight. On admission, complains of great debility and weariness of his limbs; is in a drowsy state: when roused, he talks coherently for a little, and then begins to ramble. Expression of face dull and languid; eyes suffused and heavy; lips dry; skin moist, covered with a slight, ill-defined rash; tongue moist, red at tip—covered with brown fur at dorsum; much thirst; no appetite; bowels confined; urine pale and clear; respiration hurried; a frequent cough, with some expectoration. Pulse 116—jerking, feeble; sleeps badly.

Prescription—Bryon., 3, gtt. ii. 3^{ua} qq. h.

Feb. 7th.—Has passed a quiet night, but sometimes talks incoherently. He now lies in a stupid state, answering questions incoherently. Pulse 116—jerking, soft; tongue becoming dry, with brown fur; lips dry; much thirst; bowels open; motions yellow and watery.

Prescription—Rhus., 3, gtt. ii. 4^{ua} qq. h.

* Rapou—"Treatise on Typhus Fever."—*British Journal of Homœopathy*, vol. xi., p. 652.

8th.—Much the same, except that the tongue is now quite dry.

Continue medicine.

9th.—Continues in the same lethargic state, sometimes talking incoherently; but, when spoken to in a clear voice, answers rationally; respiration hurried. Pulse 116, soft, and feeble; tongue coated with dry, brown fur; teeth becoming covered with sordes; lips dry; much thirst; bowels relaxed; motions watery, of an ochre-yellow colour; slight tenderness of abdomen.

Continue medicine. To have ℥ij. of wine.

10th.—Much the same, except that the tongue is dry, and becoming almost black; petechiæ more numerous all over the body.

Continue medicine.

11th.—Lips and teeth dry, with black sordes; tongue very dry and black; respiration 50 in a minute; skin hot and moist. Pulse 116, feeble; less thirst; takes his beef-tea and wine without difficulty.

12th.—Pulse 100; slept well, with little delirium, in the night; no delirium since; tongue moist, and less furred.

13th.—Has passed a good night. Pulse 100; quite rational.

Continue medicine.

14th.—Passed a sleepless night ; more incoherence ; tongue dark, and very dry ; restless ; bowels not moved ; skin moist ; some perspiration.

Prescription—Bellad., 3, gtt. ii. ter die. Coffea, 3, gtt. ii. at night.

15th.—Slept much better ; no other change.

Continue medicine.*

17th.—Has passed a pretty good night ; talks incoherently ; tongue dry, and covered with brown fur. Pulse feeble ; takes food and wine well ; bowels are now regular.

Continue.

19th.—Says he is much better ; lies on his side, and speaks rationally ; expression of face natural ; respiration tranquil ; no cough. Pulse 88 ; tongue moist, but slightly furred ; bowels open ; motions natural.

From this time convalescence set in, and he advanced steadily to perfect recovery.

Here we have a good example of what would be called, on the continent, *abdominal typhus*. For some days this man was in great danger. So far as we could judge, the action of the Rhus was highly beneficial. The improvement in all the symptoms on the 12th was very decided, and, in part, attributable to the medicine. It is unusual to meet with a relapse in typhus fever, as there was in this case. It may be, however, that it was not a true relapse, but an imper-

fect cure; and that, but for the Rhus, the case would have gone on steadily getting worse till the 14th, instead of mending on the 11th. I think this highly probable. In the subsequent sleeplessness, which is more common in gastric than in typhus fever, Coffea had a marked effect.

Rhus Toxicodendron was employed by Hahnemann, with marked success, in the treatment of the great epidemic of fever which occurred in the year 1813. In the introduction to the proving of this medicine, Hahnemann states that he treated 183 cases of typhus fever, at Leipzig, without the occurrence of one single death. "By the administration of Rhus, *in alternation* with Bryonia, it was possible to cure all these cases." For particulars, he refers to an article which appeared in a periodical of the day. Here we meet with a description of the fever, and the method of its treatment, as follows:—"This fever has two principal stages. In the first period, which is short in proportion to the severity of the attack, there are—increased sensation of the pains usually present, with intolerably bad humour; sensation of heat in the body, and especially in the head; dryness, or sensation of this, in the mouth, causing constant thirst; bruised feeling in the limbs; restlessness, &c.: but, in the second period—that of delirium (a metastasis of the whole disease upon the cerebral organs)—there is no complaint of any of these symptoms: the patient is hot, but does not desire to drink; he knows not whether to take this or that; he does not recognise persons about him, or he abuses them; he makes

irrelevant answers; talks nonsense with his eyes open; does foolish things; wishes to run away; cries aloud, or whines, without being able to say why he does so; has a rattling in the throat; the countenance is distorted; the eyes squinting; he plays with his hands; behaves like a madman; passes his fæces unconsciously, &c.” “In the first period two medicines are of use—Bryonia and Rhus. If the patient complains of dizziness, *shooting* (or jerking, tearing) pains in the head, throat, chest, abdomen, which are felt *particularly on moving the part*, in addition to the other symptoms—the hæmorrhages, the vomiting, the heat, the thirst, the nocturnal restlessness, &c.—we give him a single drop of the 12th dilution in the morning. Improvement takes place in the course of twenty-four hours; and, as long as this lasts, we give him no other medicine, nor even repeat this one. If now the amendment, produced by the single dose of Bryonia, goes off in the course of two, three, or four days—that is to say, if the patient then complains of *shooting pains in one or other part of the body whilst the part is at rest*—if the prostration and anorexia are greater; if there is harassing cough, or such a debility of certain parts as to threaten paralysis—we give a single drop of the tincture of Rhus Toxicodendron, prepared in the same way. * * *

The whole disease will generally be removed by a single drop of the second or of the first medicine (according as the one or the other is indicated, without the addition of any other).” I may here observe, *en passant*, that these medicines were not given in

alternation, or alternate succession, as one is led to suppose by the description of the treatment given in the introduction to Rhus; but *alternatively*, as one or other set of symptoms preponderated. Hahnemann continues, and puts the observation in italics:—" *Rhus is suitable more frequently than Bryonia, and hence can be more frequently used at first alone.*"*

It would be very interesting to read the account of the fever treated by Hahnemann, given by Hufeland in a small book, entitled *Über die Kriesgspest alter und neuer zeit mit besonderer Rücksicht auf die Epedemie des Jahres, 1813, in Teutschland*. It is not in the British Museum. But in the volume of the *Edinburgh Medical and Surgical Journal* for the year 1817,† there is a pretty full review of it; in which we are told that, "In December, 1812, and in January, 1813, the miserable remains of the immense French army, which shortly before had bid defiance to the whole world, but now almost destroyed by cold, *hunger*, and misery, reappeared on the frontiers of Prussia. More dead than alive; debilitated in the extreme; emaciated; physically and morally depressed; partly with frost-bit limbs—the former conquerors of the world first appeared in Lithuania and Silesia. Never has the world seen such a mass of misery collected in one place; and never were its effects so dreadfully felt. They sank down with general trembling; total exhaustion of power; failure of the mind and

* See *Allgenn. Anzeig. der Deutschen*. Hahnemann's *Lesser Writings*, translated by Dudgeon; p. 712.

† See *Edinburgh Medical and Surgical Journal*, vol. xiii.

nervous system; death of the skin, with fever, petechiæ, and colliquative diarrhœa." Here we have the description rather of what is called famine fever than typhus proper. To this, however, there succeeded a much more virulent form of the disease, which raged chiefly among the Saxon garrison of Torgau, but spread generally over the chief towns in Saxony and the Prussian frontiers. Of the combined effects of the two, no less than 30,000 of the French troops alone perished in four months.

The 183 cases treated by Hahnemann so successfully, probably belonged to the first class. Famine fever we know to be by no means a deadly disease when properly managed. Out of 441 cases admitted into the London Fever Hospital, from the year 1848 to 1857, there were only eleven deaths, or about $2\frac{1}{2}$ per cent. So that there is no reason to doubt Hahnemann's statement that he succeeded in curing all the cases he treated. Nor is it at all surprising that cases of a precisely similar kind died under the hands of the other practitioners then in Leipzig; for, writing at that date (1813), of the treatment of fever, Hufeland says—"The antiphlogistic method, and especially blood-letting, now begins to be as prevalent as the tonic and stimulant plan was ten years ago." What the consequences of blood-letting in famine fever are, we all know; and to the positive mischief done by his colleagues, no doubt, is in some measure due the contrast presented by the results obtained by them to those of Hahnemann.

In the 6th volume of the *British Journal of Homœopathy*, you will find an account, by Dr. Kidd, of his

experience of the famine fever of Ireland, of 1847. The results he obtained were, on the whole, very favourable.

When Belladonna, Bryonia, and Rhus are insufficient to prevent the patient sinking into a lower depth, the medicine we then usually administer is Arsenicum. The following case may be taken in illustration:—

Susan S——, æt. 16, was admitted on the 3rd of January, 1864. Two days before her admission she was seized with great giddiness, and was taken to the Refuge, whence she was brought here.

On admission, she complains of great pain in head and limbs. The tongue is coated with a thick whitish fur; there is anorexia, but great thirst; the skin is hot and dry, and covered with an indistinct, mottled eruption. The pulse is 120. There is cough. She is very deaf. Her bowels are confined.

Prescription—Rhus, 3rd dil., gtt. ii. ter die.

Jan. 8th.—Pulse 120. Complains of great pain in head, chest, and limbs. Tongue parched.

10th.—No change.

Prescription—Arsenicum, 3rd, gtt. ij. 3rd q. h.

14th.—Seems much better. Tongue is getting moist; skin cool. Pulse 115.

Continue medicine.

18th.—Better every way.

23rdth.—Discharged, well.

The symptoms produced by slow poisoning with Arsenic, are so similar to those of typhus fever, that it has often happened, as in some cases related in the *Edinburgh Medical Journal* (No. 108), that the sufferers were treated for this disease. The report of one of the cases runs thus:—"At first sight he appears to be suffering from typhoid fever. Constant somnolence; great stupor; dull look; singing in the ears; redness of the cheeks; great emaciation, &c."

Besides the general resemblance of the symptoms of Arsenic to those of typhus, there are two special specific effects of this drug, which endow it with peculiar value. I mean its action on the heart and on the kidneys. These are the organs we require to guard with greatest care in typhus; it is through them that the deadliest blows are delivered. The action of Arsenic on the heart is so well established, that it requires only to be suggested. No less incontestable is its effects upon the kidneys. In one case we find among its symptoms—"Suppression of urine, lasting seven days." Now, as we know that the most fatal of all the complications of typhus is convulsions arising chiefly from paralysis of the kidneys, the importance of a medicine which includes in its sphere of operation a powerful action on the kidneys, is sufficiently obvious.

The next case to which I wish to direct your attention, is one of a mixture of true typhus and enteric fever.

Harriet M —, aged 43, was admitted on the 5th of June, 1862. Has a son and daughter in hospital, convalescent from typhus fever. On the 1st-

i. e., five days ago—she began to suffer from shiverings, and pains in the limbs and head. On the 3rd she was obliged to keep her bed.

On admission, she complains of frequent chilliness; pain in limbs; headache; dry cough, with pain across chest. Bowels relaxed; stools watery, with a sediment like ochre. There is some tenderness in left iliac region. The expression of the face is anxious; the lips are dry; the respiration accelerated; skin hot and dry. Pulse 122, of moderate volume; tongue moist, coated with a yellow fur; breath offensive; much thirst; sleeps badly. Some elevated, pinkish spots, which disappear on pressure, are observed; also some pale-brown eruptions, level with the skin.

Prescription—Bryonia, 3rd dil., gtt. ii. 4th qq. h.

June 9th.—No headache; no sleep last night; the tongue is dry and brown at the centre—red at the edges. Pulse 120, small and weak; bowels continue relaxed.

Prescription—Arsen., 4, gtt. ii. 4th qq. h.

11th.—Pulse 130; bowels moved once; motion solid; great thirst; urine high-coloured; respiration oppressed.

Phosphorus, 2, gtt. ii. omni horâ.

13th.—Stools very frequent, consisting of mucus and blood.

Merc. Cor., 12th dil., gtt. ii. 4th qq. h.

14th.—Better.

Continue.

15th.—Both urine and stools passed involuntarily, and, apparently, unconsciously.

Phos. Ac., 2nd, gtt. ii. 3rd qq. h.

17th.—Diarrhœa continues; low, muttering delirium.

Arsen., 3, gtt. ii.

Bryon., 3, gtt. ii. 4th qq. h. alternately.

18th.—Pulse 96; general improvement.

Continue.

From this time the patient gradually improved. She got no more medicine after the 23rd of June. Was discharged, well, on the 8th of July.

This, probably, would be generally considered to have been a case of pure enteric or pythogenic fever; and certainly it differs widely, in many important features, from most of the recent cases of typhus, chiefly in this—that the poison took effect upon the intestines primarily, and upon the brain and nervous tissue only in a secondary degree. Considering the great height of the pulse (130), there was little delirium, and it came on late; and although there was great weakness, there was no subsultus tendinum, or other symptom indicating any profound impression on the nerves. On the other hand, we met with the most dangerous, and often unmanageable, condi-

tion of the bowels. The patient was for some days in the greatest danger. You may observe that she got no wine. In pure typhus the rule is to give wine; but whether it should be given in cases like the above is doubtful. At all events, this patient recovered without it. We observe improvement in the bowels after the administration of Arsenic on the 9th; for, on the 11th, the stools became solid. The pulse rose, however, and there was more oppression of the chest, which led us to Phosphorus. Again, on the 13th, the stools became not only more frequent, but mixed with blood. The case at this point was very critical; but, under Mercurius Corrosivus, we had marked improvement for two days. The special indication for this medicine was the appearance of blood in the stools. However, although this particular symptom disappeared, yet, on the 17th, we find that the former state of the bowels returned, accompanied with low, muttering delirium, and great exhaustion. For this Arsenic and Bryonia were prescribed; and, from that time, rapid and steady improvement set in, and advanced to convalescence.

That two specific morbid actions could occur simultaneously in the human body, and each pursue its course without arresting or modifying that of the other, is a proposition entirely at variance with the opinions of the old pathologists, who flourished before the time of Hahnemann. They held, generally, the doctrine taught by Hunter, that no two specific diseases can co-exist in the animal economy. On this presumption Hahnemann founded part of his

explanation of the curative action of medicine; and from this he deduced some of his rules against the administration of a second medicine, till the period of the operation of a previous one had entirely expired. It is of great importance to us to know whether this pathological doctrine be in accordance with the larger experience and observation to which we now have access. We could hardly have two diseases more specifically distinct than typhus and small-pox. Yet, in the following narrative, we have a description of the two running their course, side by side, without either interfering with the other. It occurred in the London Fever Hospital, under the care of Dr. Buchanan; and is quoted by Dr. Murchison, at page 209 of his work.

“A girl, aged 15, was seized, on June 1st, 1862, with severe pains in the back, vomiting, and loss of appetite; followed by an eruption of variolous papules on June 3rd. On June 6th she was removed to the Small-pox Hospital, where the symptoms ran the usual course of a mild attack of variola, modified by vaccination. There were good cow-pox marks on the arm. The febrile symptoms, however, did not recede; and, on June 11th, a typhus rash made its appearance on the trunk. On June 12th she was removed to the Fever Hospital: and at this date there were a number of desiccating pustules on the face, together with a well-marked typhus rash on the chest and abdomen. This rash was still distinct on June 18th, but disappeared on the following day, and the patient made a good recovery. Several small-pox cases had occurred

in the next house to that where the girl was taken ill, and there was also much typhus in the neighbourhood. The girl had also been removed to the Small-pox Hospital in a carriage used to convey typhus patients."

This is certainly a most interesting case. We cannot question the competence of the observers, or the substantial accuracy of the statements; and we find that, on the 12th of June, there was a well-marked typhus eruption visible. Now, four days is about the earliest period, from the time of infection, at which so distinct an eruption is manifest. This would give the 8th of June as the latest date for the reception of the typhus poison. The small-pox eruption appeared in its first, or papular form, on the 3rd of June; so that upon the fifth day of the small-pox—that is, during its greatest intensity—she was infectable, and actually infected, by another wholly different specific morbid poison. There are many instances of a similar kind now on record; and we must frame our theories so that they shall embrace this new category of cases. Suppose we encounter a case (and there are such on record) of a combination of scarlet fever and of typhus; how are we to deal with it? Are we to engage only one of the two destroying agents, and let the other alone till the first be entirely subdued? If we believe it is impossible for two medicinal actions to proceed, *pari passu*, in the animal economy, this is the rational course to pursue, and the one recommended by Hahnemann. But if two entirely different *natural* morbid processes can co-exist in the human body, without the one affecting the development of the other, what

reason is there why there may not be two artificial simultaneous series of morbid phenomenon, each equally independent of the other? If scarlet fever, or if small-pox, can each run its course, while at the same time typhus is doing so likewise, why may not Belladonna and Arsenicum each run their course when given in alternation? I confess, for my part, I can see no reason why they should not; and I believe we have now an overwhelming amount of evidence in favour of giving, in a certain class of cases, two medicines in alternation. The objection usually urged against this method is, that it leads to laxity of practice. True, if we give two medicines instead of one, and let the system take its choice, as it were, to which it shall submit. If in a given case, for example, we find two medicines pretty nearly indicated, and, instead of ourselves selecting the one and rejecting the other, we toss them both in, trusting that the right one only will act, and the other be a nonentity, or negative quantity. No one who deals conscientiously with himself will deliberately approve of so simple a method of evading the difficulty of choice. But it is wholly different, if, seeing before us two distinct morbid actions, which we know to be capable of separate and independent progress, we arrest the one by its specific antidote, and administer another remedy to counteract the other. To prevent the possibility of being misunderstood in reference to the important matter, let me observe, that the administration of medicines in succession, or in alternation, is wholly different from giving such medicines combined. What

the effects would be of mixing Aconite and Bryonia, can only be known by a series of experiments, to ascertain the properties of the *tertium quid* thus formed. The most perfect knowledge of the properties of Muriatic Acid and of Soda, separately, never could have revealed those of common salt.

This is a matter which demands settlement—demands the adjustment of practice to theory. Nothing can be worse than persisting in doing what we profess to be wrong. Let us come to some decision upon it. My opinion is, that if we boldly and honestly undertake the investigation of this question without prejudice, we shall arrive at the conclusion, that there are medicines which may be given in succession or alternation, without the action of the one interfering with that of the other; and that it is impossible to do justice to our patients, if, misled by doctrines founded upon antiquated and exploded pathological dogmas, we deny ourselves the use of more than one remedy at a time, from the belief that two medicines cannot act curatively without the action of the one in some way interfering with that of the other. I believe this notion is an error; and if so, it is high time it should be recognised as such. I believe we have been forced, by the exigencies of practice, for a long time to act against our theories; and that the result is, we have arrived at the point when we must square our theories with our practice, and not our practice to our theories. •If it be true that medicines may act simultaneously without mutual interference, a new field of investigation lies open; and if so much has

been achieved by the elder branch of the house of Hahnemann, who restricted their efforts to obtain their end by single medicines, we have a right to expect a great expansion of the powers of his system, when we shall have ascertained, not only the relations of medicines to their respective morbid conditions, but their inter-relations to one another, and how their various powers may be combined to produce one common effect. The elder branch introduced melody into medicine; but it may be for the younger branch to arrange the melodies, and thus arrive at harmony. In dealing with a deadly fever, half our usefulness would be paralysed if we were forbidden to treat an inter-current morbid action, except on condition that we allowed the original poison to work its mischief in the system, unchecked, till the secondary attack were arrested.

I have been led to make these observations, to explain the use of Bryonia and Arsenic in the case last reported. Had it been one of pure enteric fever, possibly Arsenic alone would have been sufficient for its treatment. It seems now pretty well made out, that the *abdominal typhus* of the continent is the same affection as the enteric or pythogenic fever of Britain. Arsenic is the remedy chiefly relied upon by Dr. Fleischmann. He tells us he gave it quite alone, without any other medicine, and with very good effects, in upwards of fifty cases of typhus abdominalis. At the same time, he frankly admits that we have not yet discovered a remedy which hits and extinguishes the focus of the disease. A similar

lamentation is expressed by Dr. Drysdale, in an article which appeared in the *British Journal of Homœopathy*, last July. "It has long been felt and acknowledged by most Homœopathic practitioners, that we have not met with the success, in treating continued fever, that we meet with in other diseases, or that we have a right to expect." He then points out, that the symptoms of *Agaricus* bear so strong a resemblance to what he calls *the ataxic forms* of typhus fever, as to warrant us indulging the hope that this substance may prove a true specific for the disease: and Dr. Drysdale, enforces his recommendation by giving the details of two cases in which he prescribed *Agaricus* with success. As the cases are not long, and highly interesting, I quote them in full.

"CASE I.—Under the care of Dr. Hayward.

"Mr. C——, æt. 30, of nervo-lymphatic temperament, a teetotalter and commercial traveller, had rheumatic fever some years ago, followed by sciatica, for which he was treated in the infirmary.

"Seen March 8th, 1863. Six days ago, after exposure to cold while perspiring, he became chilled; and this was followed by burning heat, dryness of throat, slight cough, great thirst, headache, loss of appetite, and great weakness, and mental and bodily prostration. He put himself under allopathic treatment, but all the symptoms gradually increased.

"*Present State*—March 8th.—Eyes heavy, suffused, and bloodshot; he appears stupid, and very weak and tremulous; skin hot and dry; tongue moist, but thickly coated with brown fur. Pulse 120, soft, and

very weak. He complains of dull, heavy headache; no appetite; great thirst; feverish heat, alternating with chills; aching rheumatic pains through the body; and that his senses are dull, and memory bad, especially for numbers and dates.—To have wine and beef-tea, and plenty of water; and

“Bryonia A., gtt. v. every two hours.

“To have a professional nurse.

“This treatment was persevered in for three days, but he continued to grow worse; and, on the 11th, the tongue had become dry and brown at the centre; he was more stupid, and tremulous, and delirious; and he refused wine. He was becoming deaf; and the pulse was 130, smaller and weaker. To have a glass of good port wine one hour, and of beef-tea the next.

“Bryonia, ϕ gtt. v. every two hours.

“This treatment was continued for three days, except that, on the 13th, he was ordered to have, every third hour, the wine replaced by two tablespoonfuls of brandy. Still he continued to grow gradually worse. The pulse became more frequent and small, and the first sound of the heart gradually disappeared. The deafness increased, and he sank down in the bed. He became more restless and delirious, and the dryness of the tongue increased; so that, at mid-day on the 14th (the twelfth day of the fever), the pulse was 140, and the first sound of the heart scarcely audible with the stethoscope; tongue very tremulous, and

quite dry and blackish, but moistenable; delirium constant; does not know his relations, and throws his wine and medicine at his nurse; hands tremulous; very restless, and delirious.

“Dr. Drysdale, who was now called in consultation, recommended *Agaricus Muscarius*, ϕ gtt. v., every two hours; and to continue the wine, brandy, and beef-tea as before. When seen in the evening he was evidently quieter, and less tremulous and delirious, and the pulse had fallen to 135; and the nurse remarked that every dose of the last medicine appeared to do him good (he had had four doses).

“The next night (the 14th) was passed much more quietly, and he had some refreshing sleep. At mid-day on the 15th, he was much more himself, and recognised his mother. Pulse had fallen to 124, and was fuller; and the first sound of the heart was returning. From this period the case went on favourably, and to perfect recovery.”

In justice to Dr. Hayward I have quoted his own unusually clear, graphic, concise, and scientific statement of the case. I feel the propriety of doing so, all the more as I am far from being convinced that the favourable result was due to the *Agaricus*. This medicine was administered at the critical hour. The patient must either have died or suddenly become better. It was impossible to say which scale of the balance was to rise. All that could be safely affirmed was, that the state of equipoise could not last. He was at the point of death or of recovery at the moment the *Agaricus* was given; and the recovery set

in. In the eyes of the nurse, this was in consequence of that medicine. Well, it may have been so ; but we should require a great accumulation of such cases to convince us ; for those who are in the habit of watching cases of typhus fever, constantly meet with equally sudden changes for the better under all or no medication. "All I insist upon," writes Dr. Stewart, "is the frequent, I may say the common, occurrence of a perceptible crisis, or what is vulgarly termed a *turn* in typhus. I think I may appeal to the experience of every physician, and, more especially, of every resident clerk in the Fever Hospital, whether they have not been struck at seeing, during their morning visits, the glassy eye ; the haggard features ; the low, muttering delirium ; the stupor, approaching to coma ; the subsultus ; the carphology ; the rapid, thready, tremulous pulse of the previous evening ; the formidable array of symptoms, in short, which seemed to indicate a speedy and fatal termination, exchanged for the clear eyes, the intelligent countenance, the steady hand, the comparatively slow and firm pulse, and the returning appetite of approaching convalescence. To such cases as these we might almost apply the Scripture phrase—'At such an hour the fever left him.'"^{*} Undoubtedly, the hour at which the fever left Dr. Hayward's patient, was that in which the Agaricus was given him. Did it leave him thus in consequence of the action of this medicine ? I am equally indisposed to answer this either in an affirmative or negative.

^{*} I have lost the reference to this quotation. °

The next and only other case treated by *Agaricus* is thus reported by Dr. Drysdale:—

“A boy, *æt.* 13, had, for a week, febrile symptoms, with catarrh. The fever was not of a decided character; moderate, and might have merely depended upon the catarrh till about the eighth day, when it put on the signs of idiopathic, continued fever; while the cough and catarrh were not worse. He then became excessively restless and tossing, and the tongue dry; constant delirium, and scarcely any sleep; hands tremulous, and *subsultus tendinum*. There was picking of the bed-clothes; and he lay almost unconscious, though he could be roused for a minute when questioned. The pupils rather contracted; no eruption on the skin of any kind; the bowels were loose, pale, and offensive. Pulse 132, small and weak. This state had lasted about two days, and he had had 3ij. of wine every four hours during that time. On the morning of the 4th of May, 1863, he was ordered five drops of pure tincture of *Agaricus* every two hours, and 3i. of brandy every two hours. In the afternoon the symptoms were much the same, with somewhat less delirium.

“May 5th.—Had a much better night, and quieter. Pulse 118 in the morning, and 104 the same evening. All the symptoms improved, and tongue moist and cleaning; the cough troublesome.—Continue medicine and brandy, and two doses of *Phosphorus*, 1, interposed.

“6th.—Pulse 90; tongue quite moist; no delirium nor *subsultus*; one stool, natural. In the night he

grew cold and faint, probably from careless nursing; but was easily revived by an additional dose of brandy. During the day he slept much, and began to take liquid food more readily. Cough troublesome.—Phos., 1, alternately every two hours, with Agaricus, ϕ .

“7th.—Appetite returning; had a good night, and no delirium. Slept much; no symptoms, but cough and debility.—Continue Phos. and Aga.

“8th.—Febrile symptoms continue better, and appetite is coming back; but the cough is more troublesome, and the pulse 100.—Stop Agaricus, and give Phos., 1, and Puls., 2, alternately every three hours.”

Improvement went on satisfactorily.

Dr. Drysdale continues—“In this case the remedy (Agaricus) seemed to have specifically quelled the typhus febrile element, and, as it were, dissected it out of the total disease, leaving the inflammatory catarrh unaffected.”

Anxious as we must all be to discover a medicine endowed with the power here ascribed to Agaricus, of quelling typhus; and placing, as we all must, the utmost confidence in Dr. Drysdale's practical sagacity; yet, I confess, this case would have been more satisfactory did we know what medicines had been administered during the eight days previous to the boy getting the first dose of Agaricus. We trust Dr. Drysdale will yet tell us this: it is really most essential that we should know whether other well-indicated H \ddot{o} meopathic medicines had failed, and, on their failure, Agaricus had been resorted to with success.

It is also unfortunate for us (though not for the patient, probably), that the *Agaricus alone* was given only one day. On the second day *Phosphorus* was added. In these circumstances we feel compelled to suspend our judgment as to the proved therapeutic value of *Agaricus*. All we can as yet do, is to accept of Dr. Drysdale's interpretation of the pathogenetic symptoms, as representing an analogue with a certain form of continued fever. I prescribed this medicine in one case, which did well; but I could not satisfy myself of any positive remedial action.

The leading characteristics for the employment of *Agaricus* seem to have been, that the delirium was a prominent and violent symptom; so, also, the trembling; the dry tongue; the small, quick, weak pulse.

The medicines we have employed in all *uncomplicated* cases of the continued fever which we have had to treat in this hospital during the late invasion of the disease, are *Aconite*, *Belladonna*, *Bryonia*, *Rhus*, *Arsenicum*, and *Agaricus*. In every single case there has been recovery; and the recovery of most of the cases has been rapid. These medicines have been given, for the most part, in low dilutions, from the first decimal to the third centesimal. Some, however, were treated by my colleague, Dr. Yeldham, with very high dilutions. As he is at present engaged in investigating the comparative merits of the various dilutions, and promises to give some lectures on the subject next session, I will say nothing upon this point beyond expressing my own satisfaction with the low dilutions in the cases under my care.

In regard to diet, the general rule has been to give beef-tea and milk and water, with from four to eight ounces of port wine. So far as my own experience goes, it is entirely in favour of giving wine freely in all cases of true typhus. I have never seen any harm come of it.

That it may have been overdone, I admit; but let us not be deterred, by its occasional abuse, from its judicious use. We have sometimes found it difficult to make patients take it; and it is in these very cases it is most required.

It is to the timely and ample supply of wine that we look for protection both to the heart and the brain; the heart, against degeneration of its structure; the brain, against protracted enfeeblement of its functions. We *know* that acute, fatty heart, probably arises from insufficient innervation of that organ. Now, it is true that alcoholic liquors may not increase the quantity of nervous matter in the heart; and that they may, by over-stimulating what there is, lead to ultimate exhaustion. At the same time, there is no doubt that they increase, during the period of their operation, the functional activity of the heart, and probably prevent the occurrence of the low and feeble vitality on which the transformation of muscle into fat depends. I admit that all stimulation is like living on capital; and, if too long indulged in, must end in ruin. But is it not better to live on capital than to die? In fever we must be even prodigal of the resources of the constitution, using all its forces to repel a deadly foe, and trusting to future accumula-

tions to meet the debt thus contracted in the hour of extremity.

Not less important is a due measure of Alcohol to preserve the integrity of the brain. As a rule, after the subsidence of the fever, the brain gradually, rapidly, and perfectly regains the condition it was in before the attack. This, however, is not always the case. In one instance under our care, the confusion of intellect remained after the complete restoration of all the other functions. The patient was an elderly woman, who acted as temporary nurse. She took the fever very slowly, so to speak; and when she had it, the delirium was almost the only symptom; so that the case had more the appearance of simple mania than true fever. There was great difficulty in getting her to take both wine and medicine; and probably she did not get enough of either. It is now well understood that the low, maniacal condition is a consequence of the starvation of the brain; and that it may, in many instances, be prevented by a judicious employment of Alcohol.

It would be a great security to fever patients if we could procure for them sleep at the early period of the attack. Sleeplessness is, in itself, very distressing, and is also very weakening to the nervous tissue. The nerves may be said to feed during sleep; as some animals eat only at night. The sleepless condition in fevers seems to be the direct effect of the poison on the brain. It is sometimes a very early symptom, and one for which, as yet, we have no sufficient remedy. I have seen good from

Belladonna and Coffea in some cases ; but I cannot speak with confidence of them ; and this we must put down as a thing to be specially sought after—a medicine of which insomnia is a leading symptom in its pathogenesis, in virtue of which it may cure that form of sleeplessness which is produced by typhus and other poisons. If we had such a medicine, we might possibly be more independent of Alcohol in the treatment of fevers ; but not having any antidote to this serious cause of derangement of the functions of the brain, we must, I apprehend, in the meantime, regard the judicious employment of wine as the best safeguard in our possession against the damage the brain incurs—first by the state of sleeplessness, and then by that of delirium, which attend most continued fevers, but especially true uncomplicated typhus.

The combination of wine with liquid nourishment is generally advisable ; for in this way, while we protect the nerves from loss, we at the same time support the muscular and other tissues during their excessive waste.

Of the complications, the most common is bronchitis : of this we have had many examples ; some in a most severe and dangerous degree. The following case, which was under the care of Dr. Hamilton, and is reported by Dr. Carfrae, is a fair average specimen :—

Mary W——, aged 47—admitted on the 6th of January, 1864—was seized, eight days ago, with “cold shivers and sweats :” that day had pains in limbs, and “all down back ;” and headache ; anorexia. On admission, complains of severe headache ; pain in back and limbs. Last night coughed incessantly ; tongue

loaded with a thick brown fur; anorexia; thirst; skin hot and dry; slight, indistinct eruption. Pulse 115; sleepless.

Rhus, 3, 3^{ua} qq. h.

January 8th.—Rather better; slept a little; cough still very troublesome; skin now free of any eruption; bowels active yesterday. Pulse 115.

Prescription—Bryonia, 3 dec., 2^{da} qq. h.

9th.—Cough very bad during the night; there is large crepitus throughout the whole chest.

Tart. Emet., 3 dec., 3^{ua} qq. h.

11th.—No better; eruptions very abundant; had a sleepless night. Complains of pains in limbs.

Phosphorus, 3 dec., gtt. ii. 3^{ua} qq. h.

12th.—Has had another sleepless night, with sickness and vomiting.

Continue medicine.

13th.—Had a better night—cough not quite so troublesome.

Continue.

18th.—Crepitus almost entirely gone. For a few days she had Arsenicum; and was discharged, quite well, upon the 30th of January.

Phosphorus seemed the medicine which did most decided good in this case. It is invaluable when there is a tendency to the hypostatic consolidation,

or typhoid pneumonia; and doubtless it is, in some measure, due to the timely administration of Phosphorus, that we are deprived of the advantage of *post-mortem* examinations. Tartar Emetic was also given in this case, but did not seem to have had any decided effect. It, too, is an invaluable medicine in the treatment of bronchitis, whether occurring in combination with typhus fever or not. In cases like the one just quoted, Dr. Fleischmann is in the habit of prescribing Senega. I have not used this much in my own practice; but I had an opportunity of observing its good effects in the Vienna Hospital; and it is a remedy we should keep in view when we have what went by the name of *pneumonia notha* to deal with—that is, a sort of passive bronchitis, attended with copious secretion of mucus, and a depressed state of the system. We may sum up the medicines on which we chiefly rely in the pulmonary affections that occur in typhus, as Bryonia, Arsenicum, Tartar Emetic, and Phosphorus. In one case in which the laryngeal symptoms predominated, Spongia was given with great benefit, and the subsequent course of the fever was very mild—whether an effect or a coincidence I could not say. This completes the list of medicines employed against the pectoral symptoms; and the result of our treatment is, that although, in a large number of the thirty patients, we had the chest affected, yet that they all made perfect recoveries, from what is truly described “as the most general cause of death from typhus in this country.”

Tartrate of Antimony is held in high esteem, by many of the old school, as a very efficient remedy in the early stages of fever; and the benefit it affords is ascribed to its emetic power. As regards its usefulness, we have the testimony of a very important witness—viz., Frederick the Great, who says—“ Even the desolation caused by war in seven great battles, did not at all equal that produced by the contagious disease in the hospital. It was a kind of ardent fever, which was accompanied with all the characteristics of a plague. The patients, in the first day of the disease, became delirious, and got boils in the neck, and under the armpits. It was of no consequence whether the physicians bled or not. Death carried off all who were attacked with the disease; and the poison was so violent, its progress so quick, and its effects so sudden, that it proved fatal in three days. All kinds of remedies were tried in vain. At last, vomiting was had recourse to, and that succeeded. Three grains of an emetic were dissolved in a measure of water, and given the patient to drink until it began to operate. This proved a specific against the disease; and, after it was had recourse to, scarcely three of the hundred of those affected died.”

Having frequently observed the most marked improvement, in conditions of the system closely allied to those produced by typhus, from the administration of Tartar Emetic in doses too small to produce even nausea, far less vomiting—it is worth enquiring whether the curative efficacy of this substance, in certain epidemics of fever, be not attributable to its *total*

action on the economy; and whether, in short, it may not deserve the title of specific given it by Frederick, in a different acceptation of the term; and whether it should not be more relied on by us, on account of its profound disturbance of the whole nervous system, than its superficial influence upon the mucous membrane of the stomach and lungs. I believe that we may cure some forms of fever by Tartar Emetic alone. I do not know that we have had any specimens of such in this hospital.

The complication second in order of frequency, and perhaps equal, in point of importance, to affections of the respiratory organs, is diarrhoea. When this occurs at a comparatively early stage, and when the stools are pale, the medicines I have generally found successful are Mercurius and Phosphoric Acid. The relief derived from Phosphoric Acid has, in some cases, been most striking. When the stools are dark, and the diarrhoea occurs at a far-advanced stage of the attack, then Arsenicum is the remedy which, on the whole, does most good. Diarrhoea more commonly attends enteric fever than true typhus; but we frequently meet with this symptom in cases which present all the features of the latter disease; and, when it occurs, it is a most formidable addition to the danger of the case. Swellings of the glands of the throat and neighbouring regions, attend the most deadly and plague-like form of typhus.

In one case under my charge here, immediate relief was given by a grain of the first trituration of Biniodide of Mercury; and this remedy I should feel disposed to

employ in similar cases, after, or in alternation with, Belladonna.

True convulsions, from suppression of urine, we know to be almost always mortal. They generally occur without warning, and hasten so rapidly to their fatal termination, as to afford us little time to attempt any measures to arrest them. If we have reason to suspect them from the diminished secretion of the kidneys, we might effect something with Terebinth and Cantharides.

We had a curious example of convulsions of a different kind, occurring in combination with true typhus; and as I have nowhere met with any account of a similar case, it may be worth while to give the notes of it before concluding.

Helen C——, aged 19, was admitted on the 5th of January, 1864. On the night of the 3rd she was seized with sickness and faintness. These symptoms were followed, on the following morning (yesterday), with giddiness and headache, which have continued ever since.

On admission, complains of pain in head and chest, Tongue foul—coated white; some thirst. Pulse 100; bowels costive.

Nux. V., 4th qq. h.

8th.—Some loose bronchial cough.

Bryonia, 4th qq. h.

11th.—Has had two or three fits after getting up

(on the 9th), with frantic excitement; otherwise, is doing well.

Continue.

16th.—Well, with the exception of pain at top of head, causing giddiness.

Belladonna, 3rd dil., ter die.

18th.—Had a bad hysterical fit last night.

19th.—Is sick; tongue coated with white fur; skin very hot. Pulse 90; bowels regular; headache.

Acon., 3rd dil., ter die.

20th.—Better.

25th.—Four days ago, a gland under the chin began to swell, and is now of considerable size, and painful.

Mercurius Sol., 6, ter die.

26th.—Much better. Discharged the same day.

The fever symptoms are but faintly indicated in this report. The girl, however, came out of the fever-nest, and had the appearance which, to those in the habit of observing the disease, seldom deceives. It was a very mild case; but this is an accident: it might have been severe; and if it had, then the convulsions, which were undoubtedly hysterical, and recognised as such, might easily have been mistaken

for those which have their origin in blood and brain-poisoning. We learn, from this, that hysteria and typhus fever are not reciprocally exclusive, as is generally supposed.

I have now mentioned all the complications we have met with; and have only to add, in conclusion, that had it not been for the two deadly ones—viz., convulsions and enormous glandular swellings—the whole thirty cases we treated would have recovered—a result which I think we may fairly, in part at least, attribute to the medication here employed.

I have but to thank you for the attention with which you have listened to these somewhat dry details, and to trust that they will lead you to prosecute the investigation of the system, open to all medical observers, in the wards of this hospital.

THE END.

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